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ART. XXXI.—*On the Gases contained in Meteorites;* by ARTHUR W. WRIGHT, Professor of Molecular Physics and Chemistry, in Yale College.

IN an article published by the writer in this Journal, for July, 1875, an account was given of an examination of the gases obtained from the meteorite of Iowa County, Iowa, which fell on February 12, 1875. This meteorite is of the ordinary stony kind, containing 12·54* per cent of nickeliferous iron, and the investigation was undertaken chiefly with a view to ascertain whether the spectrum of the gases evolved from such a body, by the application of heat, would afford any information respecting the recent theories connecting such meteorites with the comets. An analysis of the gases obtained at moderate temperatures developed the unexpected fact that their chief constituent was carbon dioxide, with a small proportion of carbonic oxide, these two gases constituting more than nine tenths of the product evolved at a temperature of 250°, and nearly one half of that given off when the heat was just below redness. As was to be expected from such a composition, the spectrum obtained from the earlier portions of gas given off was chiefly that of the carbon compounds, and showed a very close resemblance to those of several of the comets.

Among the conclusions drawn from the investigation, it was stated, that the nature of their gaseous contents establishes a marked distinction between the stony meteorites and the irons

* Analysis of Prof. J. L. Smith, this Journal, III, x, p. 362.

"hitherto examined," provided the Iowa meteorite could "be taken as a representative of its class."* With a view to obtain data for a more extended comparison, the investigation was continued. and a number of meteorites of both classes examined. The results of this work are given below, and it will be seen that they tend to justify completely the conclusions in my former paper, so far as any limited number of determinations could do so.

The method of experiment was the same as that described in the former paper, except in some of the minor details, and need be but briefly described here. The specimen to be examined was placed in a tube of very hard and refractory glass, which was merely softened at a red heat, and which, when filled with the meteoritic substance, could be maintained for a long time at this temperature without yielding more than so much as merely to deform the tube. In no instance was air admitted by the cracking or drawing in of the hot glass. The air was exhausted and the gas collected by means of a Sprengel pump of such perfection that it would produce a vacuum of but a fraction of a millimeter, and maintain it for days unchanged. The specimen tube having been attached to the pump, the latter was set in action and kept running until the air was thoroughly removed, as could be seen by the state of the gauge. The meteorite was then heated cautiously and the gas pumped out into the tube in which it was to be examined. Further details of the mode of procedure, where varied in the different cases, will be given in their appropriate places.

The problem of determining the exact nature and relative proportion of the gases in a meteorite is less simple than it might at first sight appear. For not only, as Grüner has shown,† is metallic iron attacked by carbon di-oxide, but it also, in the presence of this gas, or other oxidizing agents, determines the reduction of carbonic oxide, and its disappearance therefore from the gaseous products. In the case of the stony meteorites the question is still more complicated, as there is always present a greater or less quantity of oxide of iron, which at an elevated temperature must exert no inconsiderable influence upon the constitution of the gaseous mixture obtained from the mass. Grüner's very careful experiments showed that pure carbonic oxide progressively reduces the oxide of iron, at a temperature of 400° C. On the other hand it is itself reduced by metallic

* This conclusion has been criticized as hasty by Prof. J. W. Mallet (this Journal, III, x, 206), and a second one by M. M. Delafontaine, (Bibliothèque Universelle, Oct., 1875, 188), both of whom have overlooked or ignored the fact that they were given as merely provisional, conditioned upon the assumed general agreement of other iron and stony meteorites, as respects the gases derived from them, with those to which the statement referred.

† Comptes Rendus, lxxxiii, p. 28, et al., lxxiv, p. 231, etc.

iron, with a deposition of pulverulent carbon, though the action is very slight at temperatures less than 400° C. The commission who reported upon his memoir, in repeating some of his experiments, found that the temperature must exceed 350° in order that this effect may be produced at all. At higher temperatures the action is very marked. More recently Sir I. Lowthian Bell, in his work containing the results of a very elaborate and admirable series of researches upon the mutual action of the two oxides of carbon in the presence of metallic iron and oxide of iron,* has, in the main, confirmed Grüner's conclusions, but has shown that the results vary, not only with the temperatures, but also with the relative proportion of these substances present. He found that pure carbonic oxide begins to reduce Fe_2O_3 at from 140° to 200° C., according to the substance used, while at a moderate red heat the oxygen is rapidly removed, with the formation of carbon di-oxide. On the other hand the latter gas was partially reduced by spongy iron at a low red heat, with the formation of carbonic oxide. We have further to consider the action of the hygroscopic moisture upon the metallic iron, as well as the mutual action of hydrogen and oxide of iron, at elevated temperatures.

It is very evident then that the composition of the gases obtained at or above the temperature of red heat cannot be considered to represent accurately the true constitution of the gaseous contents of a meteorite, and especially is this true in the case of the stony ones. On the other hand we can hardly assert with confidence that the different gases are expelled in exactly their proportionate amounts at all temperatures. In fact the experiments show that the proportions of the gases vary with the temperatures of their evolution in a manner not satisfactorily explainable on the assumption that such an effect is due to chemical action alone. It is important therefore that the experiments should be conducted in such a way as to facilitate as much as possible the evolution of the gases, while at the same time they are exposed for as short time as possible to the action of high temperatures. The first of these conditions is attained in a good degree by reducing the material examined to a state of minute subdivision. The second is approximated by continuing the application of the high temperatures for the shortest time consistent with a satisfactory effect in driving off the gases sought.

In the case of the iron meteorites the material was generally prepared by boring out the solid iron with a steel drill upon a lathe, the substance being rendered as fine as possible. In

* Chemical Phenomena of Iron Smelting.

some instances this was not practicable from deficiency of material, and chips produced by a planing machine were used. The stony meteorites were reduced to powder in a diamond mortar. The iron contained in them being for the most part in very minute particles no further operation was necessary in this case. The powder from the irons, when the tube containing it was deprived of air, gave off a small quantity of gas from the mere diminution of pressure, without the application of heat, in one instance enough having been evolved to allow of its collection in a tube. A qualitative examination of it showed that hydrogen and the oxides of carbon were present, leaving no doubt that the mere pulverization of the iron was sufficient to cause it to part with a portion of its gaseous contents at ordinary temperatures, and greatly to facilitate the process at higher temperatures.

The heat was applied by means of a Bunsen burner, carried slowly back and forth beneath the tube, which was wrapped with wire gauze. For the irons the temperature was carried, in the first instance, to a point below redness, in order that the action of the iron upon the gases should be as little as possible. It was about 500° C. The gauge was watched during the heating and, as soon as it ceased to rise perceptibly, the flame was slowly withdrawn, and the gas at once pumped out. The evolution of gas, at this temperature, generally ceased very nearly in twenty or thirty minutes. After the gas was thoroughly removed, the iron was heated to redness with a cluster of four Bunsen burners, the heat being continued as long as any considerable amount of gas appeared to come away. This required usually but thirty or forty minutes, though in one or two instances it was continued somewhat longer. It will be seen from the results given below that the larger portion of the gas was obtained at the lowest temperature, in every instance but one.

The iron meteorites examined were the following: First, that from Tazewell Co., Tennessee, described by Professor J. L. Smith, in this Journal, II, xix, 153. Its composition is Fe, 83·02; Ni, 14·62; other substances, 1·93. No carbon was found. Specific gravity 7·9.

Second, that of Shingle Springs, Eldorado Co., California, described by Professor B. Silliman, this Journal, III, vi, 1. It contains Fe, 81·48; Ni, 17·17; C, 0·07, other substances, 1·27. Sp. gr. 7·875.

Third, the meteorite of Arva, in Hungary, noticed in this Journal, II, viii, 439. The analysis of A. Löwe gives Fe, 90·471; Ni, 7·321; residuum of carbon, silica, and cobalt, 1·404. Sp. gr. 7·814. Another analysis by Bergemann, Pogg. Ann., c. 256, gives for its composition, exclusive of the sulphide of iron contained in it, Fe, 82·25; Ni, 8·12; Co, 0·364; P, 0·74; C, 1·54; Graphite, 2·00.

Fourth, the great Texas meteorite in the cabinet of Yale College, described by Professor C. U. Shepard, this Journal, I, xvi, 216, also, with an analysis, by Professors B. Silliman, and T. Sterry Hunt, this Journal II, ii, 370. It contains Fe, 90·91; Ni, 8·46; residue containing carbon, 0·50. Sp. gr. 7·543.

Fifth, that from Dickson Co., Tennessee, described by Professor J. L Smith, this Journal, III, x, 349, and examined at his request. It contained Fe, 91·15; Ni, 8·01; Co, 0·72; Cu, 0·06. Sp. gr. 7·717.

The following table gives the results obtained, the numbers expressing parts in one hundred. The numbers in the third line in each case give the percentage of each gas in the total amount obtained. They are not the simple averages of the numbers above them, but the means reduced according to the volumes in each case. The totals in the last column are the sums of the volumes given off at the different temperatures.

Name.	Temperature.	CO_2.	CO.	H.	N.	Volumes.
Tazewell	Co., 500°,	18·34	38·45	41·51	1·70	1·87
	Red heat,	7·76	45·75	44·76	1·73	1·30
	Total,	14·40	41·23	42·66	1·71	3·17
Shingle Springs,	500°,	19·98	13·52	60·92	5·58	0·65
	Red heat,	1·10	10·39	84·40	4·11	0·32
	Total,	13·64	12·47	68·81	5·08	0·97
Arva,	500°,	18·20	38·72	40·62	2·46	8·89
	Red heat,	11·25	74·59	12·84	1·32	38·24
	Total,	12·56	67·71	18·19	1·54	47·13
Texas,	500°,	9·76	8·43	81·81	....	1·10
	Red heat,	2·18	48·58	49·24	....	0·19
	Total,	8·59	14·62	76·79	....	1·29
Dickson Co.,	Total,	13·30	15·30	71·40	....	2·2

The small quantity of the iron available in the examination of the Dickson Co. meteorite rendered it necessary to be content with a single heating to redness. The iron was in the form of coarse chips which were cut by a planing tool. The same was true of the Shingle Springs iron, and this accounts in part for the smaller volume of gases obtained in these two cases.

We may add to this list the Lenarto iron examined by Professor Graham,* and the meteorite of Augusta Co., Virginia, the gases from which were analyzed by Professor J. W. Mallet.† The former yielded CO, 4·46; H, 85·68; N, 9·86, the whole amount of gas being 2·85 times the volume of the iron. The

* Proc. Royal Soc., xv, 502. † Ibid., xx, 365.

latter gave CO_2, 9·75: CO, 38·33; H, 35·83; N, 16·09, and 3·17 volumes of gas in all. In both these instances the iron was very strongly heated, the temperature in the latter case being carried nearly to whiteness, and continued for several hours. The volume of gas was divided into three parts, and the portions obtained at the beginning. middle, and end of the operation separately analyzed. Reducing the volumes given by Professor Mallet for each of the gases in these portions to parts in one hundred, we have the following numbers:

	CO_2.	CO.	H.	N.
Beginning,	15·09	30·74	42·52	11·65
Middle,	4·23	46·12	43·64	6·01
End,	3·69	47·00	13·36	35·95

The percentages in the total amount of gas obtained are given above. It will be seen that the results for the first two portions closely resemble those given for the Tennessee iron in the table.

In the experiments with meteorites of the stony class the same method, in general, was pursued, except that the first temperature was somewhat lower, being about 350°. This was adopted in order to lessen as much as possible the chemical action of the substances upon each other, and at the same time because the relative proportions of the amounts of gas obtained at this and the higher temperature were more convenient for the analyses.

The meteorites examined were the following: First, that from Guernsey Co., Ohio, which fell on May 1, 1860, and is described by Professor J. L. Smith, in this Journal, II, xxxi, 87. It contains 10·7 per cent of nickeliferous iron, and has a specific gravity of 3·55.

Second, one from Pultusk, in Poland, which fell on January 30, 1868. This was subjected to an elaborate investigation, and described, by Dr. G. vom Rath.* Several thousand small masses were collected, of which, some examined by vom Rath were found to contain 10·06 per cent of nickeliferous iron, though other specimens analyzed by Werther and Rammelsberg gave 21·08, and 21·78 per cent respectively.† It resembles somewhat the Iowa stone in its general character, and has a specific gravity of 3·725. The writer is indebted to the courtesy of Professor G. J. Brush, who sacrificed an excellent specimen for the examination, from his private cabinet.

Third, the meteorite of Parnallee, India, Feb. 28, 1857, found by Pfeiffer‡ to contain 6·84 per cent of meteoric iron. It has a specific gravity of 3·44.

* Festschrift der Niederrheinischen Gesellschaft für Natur- und Heilkunde zum 50jährigen Jubiläum der Universität Bonn.

† C. Rammelsberg, Die chemische Natur der Meteoriten. Abhandlungen der Königl. Akad. der Wissenschaften zu Berlin, 1870.

‡ Wien. Akad. Ber. 47.

Fourth, the meteorite of Weston, Conn., which fell Dec. 14, 1807. This is one of the most interesting meteorites known, and is remarkable both for its lithological character, and for the large amount of iron contained in it, this being estimated as from 30 to 40 per cent. Its specific gravity is 3·6.* These, together with the Iowa County meteorite, all belong to the class of chrondrites, of G. Rose, or sporadosidères, of Daubrée, and are good representatives of the ordinary or most numerous class of the stony meteorites.

In the examination of the Iowa County meteorite already referred to, the determinations were made for a number of different temperatures, the results being as follows:

	At 100°.	At 250°.	Below red heat.	At low red heat.	At full red heat.
CO_2	95·46	92·32	42·27	35·82	5·56
CO	0·00?	1·82	5·11	0·49	0·00
H	4·54	5·86	48·06	58·51	87·53
N	0·00	0·00	4·56	5·18	6·91
	100·00	100·00	100·00	100·00	100·00

The separation of the gaseous volume into so many small portions rendered the estimation of minute quantities of any constituent less certain, and it is probable that the percentage of the nitrogen, which was estimated as a residue, may have had thus set down to it, besides the errors of the determinations, very small amounts of carbonic oxide and possibly of marsh gas, which was found in all the cases in the present investigation. But they were at all events too small to be certainly distinguished from errors of observation. In the re-examination of this meteorite for carbon di-oxide mentioned below, the nitrogen was directly determined in gas given off after exposure to a red heat for a considerable time, and corresponding nearly to the portion referred to in the last column of the above table. The amount found was 3·41 per cent. But no great stress should be laid upon such a discrepancy, considering the manner and the purpose of the preceding determination. The latter determination agreed with the former as to the absence of carbonic oxide and marsh gas, at that temperature.

The results obtained for the different cases are shown in the following table. The numbers given for the Iowa County meteorite are reduced from the former analysis, the volumes being obtained from the notes made at the time, and 500° being assumed as approximately representing the temperature there given as "below red heat." The first temperature in the case of the Ohio meteorite was also 500°, this being the first one determined. The second heating was also continued for a longer time, which accounts for a slight difference between this

* B. Silliman, Sen., Memoirs Conn. Acad., vol. i, p. 142.

and the other cases. As it was found that this degree of heating left too small a proportion of the gas for the second determination, as also for other reasons mentioned above, the temperature of about 350° was employed in the succeeding experiments:

Name.	Temperature.	CO_2.	CO.	CH_4.	H.	N.	Volumes.
Ohio,	500°,	82·28	2·16	2·26	12·37	0·93	2·06
	Red heat,	16·79	8·71	1·66	69·43	3·41	0·93
	Total,	59·88	4·40	2·05	31·89	1·78	2·99
Pultusk,	350°,	81·01	1·99	1·73	13·36	1·91	0·99
	Red heat,	33·97	7·35	6·00	49·99	2·69	0·76
	Total,	60·29	4·35	3·61	29·50	2·25	1·75
Parnallee,	350°,	87·53	1·13	1·22	8·72	1·40	1·56
	Red heat,	72·43	2·53	3·22	20·03	1·79	1·17
	Total,	81·02	1·74	2·08	13·59	1·57	2·63
Weston,	350°,	86·29	1·84	1·19	8·59	2·09	2·69
	Red heat,	62·18	3·43	3·10	28·16	3·13	0·80
	Total,	80·78	2·20	1·63	13·06	2·33	3·49
Iowa,	500°,	58·04	4·01	0·0	34·82	3·13	1·04
	Red heat,	19·16	0·21	0·0	74·49	6·14	1·46
	Total,	35·44	1·80	0·0	57·88	4·88	2·5

The heat was continued, in the case of the Iowa meteorite, longer than in the subsequent experiments with the others, and the result shows a greater diminution in the amount of carbon di-oxide obtained. Rejecting the last column in the analysis quoted above, we have for the total average percentage up to red heat, CO_2, 49·51; CO, 2·64; H, 43·93; N, 3·92, which corresponds more nearly with the results in the other cases. The numbers given in this table show a very satisfactory concordance, though there are slight differences, doubtless arising from the fact that the temperatures employed, and the times of exposure to the heat, though approximately the same in the different instances, could not be made absolutely identical. The mass of material operated upon was also not always the same, which would produce a slight difference in the time required for the evolution of the gas, and the completeness of its elimination.

It will be observed that a small amount of marsh gas was found in each of the portions of gas obtained in the present investigation. This might possibly be accounted for, in the case of the higher temperatures, by the decomposition of organic matter taken up by the meteorite subsequently to its fall, or of carbonaceous matter originally contained in it; but as such decomposition would not be likely to take place, to any great extent, at so low a tempera-

ture as 350°, there is reason for believing that it is really one of the constituents of the meteoritic gases. The determinations made both by absorption of the carbonic oxide with cuprous chloride, and by the production of carbon di-oxide after the explosion with oxygen, agreed very well, and the analyses in each case were best satisfied by the assumption of the amounts of marsh gas indicated in the table. The Ohio meteorite was also examined at a number of different temperatures, the different portions of gas having the following proportions of carbon di-oxide: at 100°, 95·92; at 250°, 86·36; at 500°, 82·28; at incipient red heat, 33·55; at red heat, 19·16, showing a progressive decrease similar to that observed in the case of the Iowa meteorite.

On comparing the results given in the two tables a marked difference is at once evident. Not only do the stony meteorites give off a much larger volume of gas at low temperatures, but the composition of it is in all the cases examined quite distinct from that of the gas evolved from the irons. In no case among the results obtained from the latter is the amount of carbon di-oxide greater than 20 per cent at 500°, nor than 15 per cent from the whole quantity evolved, while in every case but one the volume of carbonic oxide is considerably larger. In the chondrites, on the other hand, the percentage of the latter gas is conspicuously small, while the carbon di-oxide is more than half of the total quantity of gas obtained up to red heat, except in the case of the Iowa meteorite, and in that the percentage is not much less, especially if we reject the numbers in the last column above, for the amount obtained by a second and long-continued application of red heat. At a temperature of about 350°, it constitutes from 80 to 90 per cent of the gaseous products, in all cases, while at the heat of 100° C. it forms somewhat more than 95 per cent of the gas evolved in the only two cases examined in this respect. The hydrogen, on the other hand, progressively increases in quantity with the rise in the temperature of evolution, and in the last portions given off at red heat is generally the most important constituent. Its proportion in the total percentage would, no doubt, be considerably increased if the heat were greatly intensified, as for instance, if carried to a point approaching whiteness, but the results obtained in such a way would be entirely unreliable, from the action of the metallic iron and the oxide of iron on the carbon compounds, or upon the hydrogen itself.

In the examination of the Parnallee, Pultusk and Weston meteorites, a small quantity of the moisture given off at a high temperature was collected in a glass tube attached to the pump and surrounded with a freezing mixture. This, when tested, gave distinct traces of chlorine for the Parnallee and Weston,

but that from the Pultusk seemed to contain little or none. The latter however, as well as the Parnallee, showed the presence of a minute quantity of sulphurous oxide, the Weston meteorite less certainly.

A question naturally suggests itself as to the manner of the occurrence of the carbon di-oxide in conditions which admit of its being separated so much more readily than the other gaseous substances. The most probable supposition seems to be that it is condensed upon the fine particles of iron as well as absorbed within them. That it is produced by the decomposition of some carbonate is not likely to be the case, since the carbonates that could occur in meteorites all require high temperatures for the evolution of this gas, and the quantities obtained should increase constantly with an increase of temperature, whereas the reverse is true; and certainly none of them would give up the gas at the temperature of boiling water. Another hypothesis might be that it is absorbed in part from the atmosphere. To test this, a re-examination of the Iowa meteorite was made, the material being heated until it yielded as nearly as possible the same volume of gas as in the experiments of the preceding year, a short time after its fall. Had it been constantly gaining carbon di-oxide from the air it should have given the same amount of gas as before at a lower temperature. On the contrary it required a more intense heating, and a longer continuance of the process. The percentage of CO_2 was found to be 32·65. If any difference exists therefore it has lost rather than gained, at least in this interval of nearly a year. It is very probable therefore that no considerable part of the gas is derived from the atmosphere, though this cannot be asserted absolutely, and the question must remain for further investigation. The portions of gas from each of the stony meteorites, except the Pultusk, which was not examined, gave cometary spectra, similar to that from the Iowa specimen.

On reviewing the results of the investigation there appears no reason for modifying the conclusions arrived at in the former article. The evolution of such volumes of carbon di-oxide may well be taken as a characteristic of the stony meteorites, and its relation to the theory of comets and their trains is certainly of great significance. The further discussion of some of the results of the investigation, and certain interesting questions suggested by them, are reserved for another communication.

Yale College, March 18, 1876.

ART. XXXII.—*Review of Croll's Climate and Time with especial reference to the Physical Theories of Climate maintained therein;** by SIMON NEWCOMB.

THE present notice of Mr. Croll's work is confined to an examination of his physical theories of climate, avoiding all those portions which have a geological bearing. The physical theories propounded have two distinct applications; the one to the present climate of the earth; the other to the changes of that climate during past geological ages. In the latter department of the work the principal object is to account for the glacial epoch or epochs, the author conceiving that there may have been several such epochs. The data from which his conclusions respecting the past are derived are necessarily founded on his theories of the causes of present climate, since it is only by a thorough discussion of the way in which all climatic causes operate, and by tests of all the conclusions by a comparison with the present climate of the globe, that any safe rules can be formed for judging of the climate of the past.

We are forced to say at the outset that the physical data for forming a reliable estimate of the separate effects of various causes on climate are almost entirely wanting. The physical theory of cosmical heat is, at the present time, in a state nearly approaching the chaotic, a circumstance all the more surprising when we consider the advanced state of other departments of the theory of heat. Cournot and his successors have devoted to the mechanical theory of heat an amount of profound research which has made it a branch of the most exact of the sciences. On the other side, Melloni and his successors have done a great deal for what we may call the chemical theory of heat. Between these two lie the physical theory, as affecting climate and cosmical temperature, which has, comparatively speaking, been neglected entirely. To illustrate what we mean let us consider the temperature of the earth from the widest point of view. Practically, there is but one source from which the surface of the earth receives heat, the sun, since the quantity received from all other sources is quite insignificant in comparison. There is but one way of losing heat, by radiation into space. The temperature of the surface being in a state of permanent equilibrium, the quantity of heat radiated and reflected must be equal to the total quantity received from the sun. It is this equality which determines the mean temperature of the surface of the globe.

If the earth were not surrounded by an atmosphere, if, consequently, the amount of heat radiated from each square foot of the land, as well as from the whole surface, were equal to that

* Climate and Time in their geological relations: a theory of secular changes of the Earth's Climate. By James Croll. New York, D. Appleton & Co., 1875.

received from the sun, the problem of climate would be a quite simple one. But the atmosphere, and especially the vapors suspended in the atmosphere, exert a powerful influence in various ways. Perhaps the most general and wide-spread source of this influence may be found in the probable unequal diathermacy of the atmosphere to solar and terrestrial heat which may result in the mean temperature being higher than it would be if there were no atmosphere. To investigate this influence the first datum necessary is the mean temperature, first of the whole earth and then of its various zones, which would be maintained if there were no atmosphere. In other words, we wish to know what would be the temperature of a small solid body revolving round the sun at the mean distance of the earth, and presenting all its sides equally to the sun in rapid succession. *This temperature may be called the normal temperature of the region in which the earth is moving.*

We repeat that the foundation stone of any reliable investigation of terrestrial climate, with respect to its causes, must be a knowledge of this normal temperature. Without it we may have any quantity of material for discussion but nothing on which we can base a theory worthy of the slightest confidence. There are of course many other questions to follow it, but this is the one which the investigator of this subject meets at the very threshold of his investigation, just as the surveyor who attempts to make a geodetic measurement first meets with the question of the length of his measuring rod. Now, no stronger example of the chaotic state of the theory of cosmical heat can be given than the simple fact that not only is this normal temperature entirely unknown, but, so far as we are aware, no attempt has ever been made to determine it. What adds to our surprise is that while no one has attempted to determine what temperature a body like the earth would acquire in free space exposed to the solar rays, there have been a number of attempts to answer the experimentally impossible question what temperature such a body would acquire if the solar heat were cut off, so that the body should be exposed to stellar radiation alone, a temperature known in our books as that of space.

In justice to physicists it must be said that one step toward determining this fundamental temperature was taken many years ago. Pouillet and Herschel determined the actual quantity of heat radiated by the sun, and their results have been of the greatest value in investigating the thermal relations of the solar system. The remaining part of the problem is more laborious, but not, we conceive, more difficult.

Since Mr. Croll had not at hand the means of commencing a complete investigation of the causes on which terrestrial climate depends, his theory must, of necessity, fail to be entirely conclusive. Still it is worked out in a manner so laborious as to

render it worthy of very careful consideration, although, owing to the diffuse mode of treatment adopted, the complete mastery of his views is a very difficult task. For this reason it is not easy for the reviewer to feel sure that he is giving such a statement of the author's views as the author himself would regard as entirely satisfactory. We may say, however, in brief, that one great object of the author is to insist upon the important agencies played by ocean currents in influencing climate. Indeed, beyond the regular astronomical variation of climate with the latitude, this seems to be the only influence which he will allow to be important. The influence of the Gulf Stream in modifying the climate of Northeastern Europe receives especial attention, and his views of this influence seem quite well grounded. We had supposed the view that the warm and equable climate of that region was due to the Gulf Stream to be one universally held, although no one had attempted to render it plausible by an actual calculation of the amount of heat conveyed by that stream. This calculation Mr. Croll has made, and having reduced his own estimate of the volume of the stream to one-half, in deference to the views of some of his opponents, he shows that the amount of heat annually conveyed away by the stream is equal to the whole amount which a belt of the earth sixty-four miles broad, extending all round the equator, receives from the sun. We make the quantity a little less, but yet equivalent to more than the total amount of heat which falls on a million of square miles at the equator. Making all allowance for the uncertainties of these data, and for the fact that only one of the two branches of the Gulf Stream passes over to Northeastern Europe, it must yet be admitted that the quantity of heat which that region receives from the Gulf Stream is not an inconsiderable fraction of that which it receives from the sun.

An essential part of Mr. Croll's system is the wind theory of oceanic circulation, essential, however, to his views of the climate of the glacial period rather than to the climate of the present. This is a point on which there is some difference between Mr. Croll and his numerous opponents, especially Dr. Carpenter. Having made no examination of the views of Dr. Carpenter, we cannot pronounce them wrong, but the view maintained by Mr. Croll, that the winds are the principal causes of ocean currents seems well sustained. The direction of these currents may be materially modified by the earth's rotation, a cause which can be investigated only by mathematical methods, and until the mode of operation of this cause is fully understood, we cannot feel sure that the theory is complete. So far as we are aware Mr. Ferrel is the only mathematician who has entered upon this investigation, but Mr. Croll does not seem to have made much use of his results. The principal

support of the wind theory is found in the very obvious general correspondence between the winds and currents of the ocean, a correspondence so striking that it is difficult to see how the strongest presumption of a causal connection can be avoided. That the winds are, in a general way, amply sufficient to produce regular currents in the ocean seems to be shown by a familiar phenomenon on our Eastern coast. It is well known that the tides are there materially modified by the winds, so that the time of high water may be delayed or accelerated by an entire hour or more, and the height changed by one or more feet in consequence of a heavy wind. The effect of a wind thus determined must be the same as that of a difference of level equal to that which the wind is found to produce, and this again must be sufficient to produce a very strong surface current. Moreover, a continuous surface current must, in time, extend itself to a great depth through friction.

In thus sustaining the wind theory, we must not be understood to deny the existence of a general law of oceanic circulation which we understand to be due to Dr. Carpenter, and by which an undercurrent of cold water runs from each pole to the equator, to return as a surface current of warm water. That the mass of ice-cold water which forms the depths of the ocean came from the poles, and that to keep it cold, the supply must be constantly though slowly renewed will, we conceive, be disputed by no one. And the renewal of the water necessarily implies a surface set from the equator toward the poles. But, when we inquire whether the quantity of water thus interchanged can be so great as to give rise to the observed ocean currents, the answer is not quite clear, and the probabilities seem to incline to the negative. At the same time, we may have here an important feature among the causes which produce ocean currents, and the scientific method of investigating the subject is not by mere arguments, but by actual calculating the effect of each cause with judicial impartiality. Perhaps it would be unfair to say that Mr. Croll does not attempt to do this, but the impression left on the mind of the reader is that the "gravitation theory" of oceanic circulation is examined rather to refute it than to determine with mathematical precision what part differences of gravity between the polar and equatorial waters do really play in the phenomena in question.

While we agree with Mr. Croll in the important part he assigns to oceanic currents in modifying climate, we cannot accept the reasoning by which he attempts to prove that the corresponding influence of ærial currents is entirely insignificant. Speaking of the possible amount of heat conveyed by the upper currents, or anti-trades, from the equatorial to the polar regions, he says:

"The heated air rising off the hot burning ground of the equator, after ascending a few miles, becomes exposed to the intense cold of the upper regions of the atmosphere; it then very soon loses all its heat and returns from the equator much colder than it went thither." * * * "During all this time [while the upper current is traveling from the equator toward the poles] the air is in a region below the freezing point; and it is perfectly obvious that by the time it begins to descend it must have acquired the temperature of the region in which it has been traveling."

This passage is quoted as showing the weakness which everywhere marks Mr. Croll's reasoning on the subject of temperature. With all the care and study he has devoted to the subject, we are entirely unable to reconcile his views with the known laws of heat. The facts that the same amount of heat is given off when water freezes or vapor condenses which is necessary to melt the ice or to evaporate the water; that the amount of heat developed by the compression of air is equal to that absorbed by its expansion; that if, from any cause, heat passes very slowly from a warm body A to a cool body B, it will also pass slowly from B to A when B is the warmer; that a body cannot abstract heat from another without itself becoming warmed, belong to a class which he does not seem to bear in mind. In the passage we have quoted, he speaks of the hot air rising from the earth and becoming exposed to the intense cold of the upper regions of the atmosphere. But, what can this cold be but the coldness of the very air itself which has been rising up? If the warm air rises up into the cold air, and becomes cooled by contact with the latter, the latter must become warm by the very heat which the former loses, and if there is a continuous rising current, the whole region must take the natural temperature of the rising air. This temperature is indeed much below that which maintains at the surface, for the simple reason that air becomes cold by expansion according to a definite and well known law.

Having thus got his rising current constantly cooled off by contact with the cold air of the upper regions, it has to pass on its journey toward the poles "in a region below the freezing point." Here again the question arises whether Mr. Croll conceives that the temperature of a region can be anything materially different from the temperature of the air or other substance which fills the region. Apparently he does, for he speaks of the air "acquiring the temperature of the region," but what the difference is, or can be, he does not explain. There is such a thing as temperature expressive of the amount of radiant heat passing through a diathermanous region, but the "upper regions" are exposed to the radiation of the sun on the one side, and of the earth's lower atmosphere on the other,

and there is no proof that these do not equal the surface temperature. Having thus cooled off his upper current still farther by its passage through this "cold region," and that without the region becoming any warmer, he leaves it to find its way to the earth's surface, entirely oblivious of the fact that an amount of heat will be evolved by compression in the polar regions, or whereever the current reaches the earth again, fully equal to that which it lost when it rose from the equator. If he had treated this aërial current precisely as he did the Gulf Stream, computed the probable amount of the current, its temperature when it rose from the earth in the tropics, and again when it reached the earth in northern regions, and thus determined the amount of heat given out during its passage, his course would have been much more logical.

We do not propose to enter into the question of fact, how much of an upper current there really is passing from the tropics to the poles. But, if it is as great as is commonly supposed, it must be as powerful as ocean currents in tending to equalize the temperature of the globe. The fact that it is cold during its passage, instead of being a disadvantage, is a positive advantage, because the heat which it carries being latent in form is not liable to be dissipated by radiation.

Another proposition which the author attempts to prove, by reasoning which seems equally inconclusive, is, that the mean temperature of the ocean is greater than that of the land over the entire globe. We may examine his argument, for the reason that the proposition is a fundamental one in his theories of climate. The most natural and conclusive way of establishing such a proposition, would be by actual observations of temperature, but no attempt to do this is made. The author rests his doctrine wholly on four *a priori* reasons, which we may consider in their order.

(1.) "The ground stores up heat only by the slow process of conduction, whereas water, by the mobility of its particles, and its transparency for heat rays, especially those from the sun, becomes heated to a considerable depth rapidly. The quantity of heat stored up in the ground is thus comparatively small, while the quantity stored up in the ocean is great." We can hardly stop to criticise these sentences, implying as they do, that the rapidity with which solar heat is absorbed by a body determines its temperature, and also depends on its diathermancy, and that a body which is heated only by the slow process of conduction must be permanently colder than one into which the radiant heat of the sun can penetrate.

(2.) "The air is probably heated more rapidly by contact with the ground than with the ocean; but on the other hand it is heated far more rapidly by radiation from the ocean than from the land. The aqueous vapor of the air is to a great ex-

tent diathermanous to radiation from the ground, while it absorbs the rays from water and thus becomes heated." According to the usually received laws of heat, one body can be heated from another only when the latter is the warmer of the two, and the rapidity with which the heating process goes on depends on the difference of temperature, no matter whether the heat passes by conduction, or by radiation. If, then, the air is really heated by contact with the ground more rapidly than by contact with the ocean, it can only be because the ground is hotter than the ocean, which is directly contrary to the theory Mr. Croll is maintaining. The statement that the aqueous vapor of the air is diathermanous to radiation from land, but not to that from water, is quite new to us, and very surprising; but if it be true, Mr. Croll assigns directly contrary effects to the same cause in (1) and (2). Reasoning as in (1), he would have said that the air over the land, owing to its transparency for the heat rays from the land, becomes heated to a great height rapidly, while the air over the ocean, not being transparent, can acquire heat from the ocean only by the slow process of convection.

(3.) "The air radiates back a considerable portion of its heat, and the ocean absorbs this radiation from the air more readily than the ground does." Here we have the air giving back to the ocean the same heat which it absorbs from it, and thus heating it. Apparently, Mr. Croll thinks that air and ocean can thus alternately heat each other up to an indefinite extent, by natural radiation, without any necessity for more than a mere nest-egg of heat to start with from any outside source. (4) seems to be little more than a repetition of (2) in a different form.

Another idea of the author which calls for explanation is that solar heat absorbed by the atmosphere is entirely lost, so far as warming any region of the globe is concerned. For instance, in comparing the relative amount of heat received from the sun by the equatorial and the arctic regions, he thinks it a mistake not to allow for the fact that a greater percentage of the heat is absorbed by the atmosphere in the polar regions than at the equator. From the care he takes to subtract this percentage from the amount of heat received by the polar regions, he seems to think that the heat thus absorbed is totally lost, and does not warm the atmosphere at all. But a moment's reflection must show that as all this absorption must occur within three or four miles of the earth's surface, and probably half of it within a single mile, or two miles at most, while the arctic regions are more than 2,000 miles in diameter, it makes no difference what portion of the heat is absorbed by the atmosphere. In the one case, the atmosphere is warmed directly by

the absorption of heat, in the other, by contact with the earth; but the temperature of the region is substantially the same in either case.

We may now pass to the consideration of the author's views of the cause of the Glacial epoch, or of glacial epochs in general, as, according to his view, there must have been several of them. He maintains that such a phenomenon may be fully accounted for by the great eccentricity which the orbit of the earth is known to assume at certain very long intervals. Using Le Verrier's formulæ for the secular variation of the planetary orbits, he has computed this eccentricity for a number of periods, extending back nearly 3,000,000 years, and has thus found a number of epochs at which it was three or four times as great as at present. That, in the course of each million of years, there are from time to time such periods of great eccentricity is a well established result of the mutual gravitation of the planets, but whether the particular epochs of great and small eccentricity computed by Mr. Croll are reliable is a different question. The data for this computation are the formulæ of Le Verrier, worked out about 1845, without any correction either for the later corrections to the masses of the planets, or for the terms of the third order subsequently discussed by Le Verrier himself. The probable magnitude of these corrections is such that reliance cannot be placed upon the values of the eccentricity computed without reference to them for epochs distant by nearly a million of years. This fact, of itself, does not militate against Mr. Croll's theory, since the correct formulæ would no doubt show other epochs of great eccentricity which would entirely satisfy his conditions. The proposition with which we are more especially concerned is the general one that a great eccentricity, with the perihelion in one of the solstices will give rise to a glacial epoch in the hemisphere corresponding to that solstice, and it is the reasoning by which Mr. Croll endeavors to sustain this proposition which we next propose to examine.

The difficulty which the sustainer of this proposition encounters at the outset is the demonstration of D'Alembert that, whatever changes the earth's eccentricity and perihelion may undergo, the total amount of heat received from the sun in the course of a year is still the same for each hemisphere. Consequently, if the mean temperature of the hemisphere depends on the total amount of heat received, it must be the same for the two hemispheres, and but slightly different from the mean temperature of the present time. To understand the question clearly let us suppose the perihelion to coincide with the June solstice, so that the earth is nearest the sun in our northern summer, and farthest from it in our northern winter. Then, in this

hemisphere, considering only the heat of the sun, we shall have a short and hot summer, and a long and cold winter, while in the southern hemisphere the summer would occur when the sun was farthest off, and the winter when he was nearest, so that the rigor of both seasons would be greatly mitigated. Still, as just stated, the mean temperature due to solar heat would be the same in each hemisphere, the northern hemisphere having the hotter summer as well as the colder winter.

While admitting this equality in the total amount of heat received from the sun, Mr. Croll endeavors to show that the northern hemisphere would be colder and that there would be an accumulation of snow and ice during the long winter which would not be melted by the sun's rays during the short and hot summer. His principal arguments in favor of this view are given in Chapters II and IV. Beginning with the winter he says that the reduction in the amount of heat received from the sun during this season, owing to his greater distance, would lower the mid-winter temperature to an "enormous extent." Precisely how great a diminution of temperature he considers enormous he does not state in this connection, but in a subsequent chapter he computes a diminution in the mid-winter temperature of Great Britain sometimes amounting to more than 30° from this cause. This computation we regard as entirely untrustworthy, being founded on purely hypothetical laws with purely hypothetical data, but we need not challenge the result at present. The effect of this lowering of the temperature would be, he says, a great increase in the amount of snow which would fall during the winter. This conclusion we cannot accept. During the long cold winter the evaporation must be lessened, and hence the amount of precipitation also, unless warm and moist air is brought from the warmer regions of the globe. In this case the latent heat set free by precipitation, as well as the heat of the air itself, would mitigate the winter temperature. On the whole, we may consider the equivalent of twenty inches of solid ice to represent a very liberal estimate of the probable average amount of snow which would accumulate over any very great extent of surface during any one winter.

Mr. Croll's next point is that the presence of so much snow would lower the summer temperature, and prevent to a great extent the melting of the snow. He gives three reasons for this extraordinary proposition. In the first place the air will be cooled by radiation to the snow more rapidly than it will be heated by the sun. Of the fact that this cooling of the air would itself be necessarily accompanied by a melting of the snow he shows no consciousness. A simple calculation will show that a cooling of the air by some 30° Fahrenheit would melt the whole twenty inches of snow.

The second reason is that the rays which fall on snow and ice are to a great extent reflected back into space, the grounds of this statement being apparently the transparency of the atmosphere to the calorific rays of the sun on which he had just been insisting, and which he must have had in mind as explaining how the reflected rays can be got back into space. How great the reflecting and transmitting power of the snow and ice must be, to keep the snow unmelted all summer, may be inferred from the fact that during this perihelion summer the amount of heat received from the sun by every part of the northern hemisphere would suffice to melt from four to six inches of ice per day, over its entire surface, that is, it would suffice to melt the whole probable accumulation in three or four days. The reader can easily make a computation of the incredible reflecting power of the snow and of the unexampled transparency of the air required to keep the snow unmelted for three or four months.

The third cause of non-melting of the snow during summer is that snow and ice chill the air and condense the vapor into thick fogs which "would effectually prevent the sun's rays from reaching the earth, and the snow in consequence would remain unmelted during the entire summer." Here again, he says nothing about the latent heat set free by the condensation, nor does he say where the heat goes to which the air must lose in order to be chilled. The task of arguing with a disputant who in one breath maintains that the transparency of the air is such that the rays reflected from the snow pass freely into space, and in the next breath that thick fogs effectually prevent the rays ever reaching the snow at all, is not free from embarrassment. We can accept calmly any and every possible hypothesis respecting the properties and nature of the atmosphere which he chooses to propound, but must insist that the conclusions be drawn in accordance with the first principles of the theory of heat. We might therefore show that if the snow, air, fog, or whatever throws back the rays of the sun into space is so excellent a reflector of heat, it is a correspondingly poor radiator, and the same fog which will not be dissipated by the summer heat will not be affected by the winter's cold, and will therefore serve as a screen to prevent the radiation of heat from the earth during the winter.

Perhaps the shortest way of meeting the case is to refer to the best known facts of our own climate. Every winter a large portion of the northern hemisphere is covered with snow precisely as Mr. Croll supposes was the case during the Glacial epoch. According to his theory this accumulation of snow ought to offer a great resistance to the melting power of the sun's rays. How much resistance it does offer everyone knows. What effect the difference of astronomical conditions during

the epochs of great eccentricity might have had may be inferred from the fact that, adopting Mr. Croll's method of estimating solar temperature, the mid-summer heat of the northern hemisphere due to solar radiation, was from 40° to 50° higher than it now is. His layer of snow must therefore resist, not merely our present heats, but temperatures ranging from 100° to 150° Fahrenheit.

The remainder of the argument can be dealt with quite briefly, because it is based on the utterly fallacious results we have just described. The northern hemisphere being cooler the trade winds are thrown farther south. The Gulf Stream being caused, in part, by the trade winds, is thrown into the southern hemisphere, and thus the northern hemisphere is deprived of this latent source of heat and its temperature falls to a point far below the normal astronomical temperature. Considering separately the propositions that a cooler northern hemisphere would throw the trade winds south, and that this change in the winds would change the Gulf Stream, they both rest on too slender a basis to be worth consideration. We cannot therefore regard Mr. Croll's theory of a connection between the form and position of the earth's orbit and the Glacial epoch as having any reasonable show of foundation. The working out of such complex theories is of the less importance that there is no astronomical reason to believe that the solar radiation has been constant during a period of a million of years.

Washington, February 21, 1876.

Art. XXXIII.—*On crystals of Tourmaline with enveloped Orthoclase;* by Edward H. Williams, Jr.

[Read before the Chemical and Natural History Society of Lehigh University.]

While at Port Henry, N. Y., last July, visiting the newly constructed furnace at Cedar Point, I noticed large sized crystals of tourmaline in some heaps of quartz and feldspar in process of shipment up the lake.

The feldspar was orthoclase, and of two varieties: one white and compact; the other reddish and much weather-stained; the cleavage planes, especially the basal, were more or less covered with a film of sesquioxide of iron. The crystals of tourmaline in the quartz were simple prisms. Those in the feldspar, and in the second variety in particular, were peculiar; when the feldspathic matrix was fractured they readily separated from it, and proved to be mere shells of tourmaline filled with feldspar.

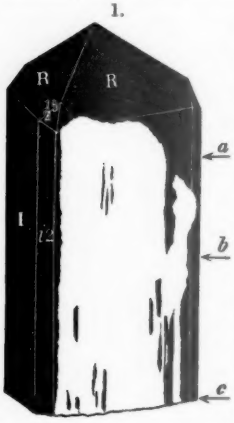
From want of time I was then unable to visit the locality from which they were taken. When again at Port Henry I

found that the minerals came from a quarry worked by Messrs. Roe and Witherbee, five miles west of the town, and about 1500 feet above the lake.

The tourmaline which presented the peculiarity was mainly in the reddish feldspar, that in the white variety being, as a rule, solid. The tourmaline occurs in long prisms with rarely more than one termination. The observed faces are: Rhombohedral, $\frac{1}{2}$, $1(R)$, $-\frac{1}{2}$; scalenohedral, $-\frac{1}{2}^{5}$; prisms, I, i-2, i-$\frac{5}{4}$. The common form is shown in Fig. 1. The crystals are commonly distorted, and are frequently terminated by but a single rhombohedral plane. Specific gravity, 3·11. Fuses before the blowpipe easily, with intumescence, to a dark bead.

In the specimens obtained, there seem to be two varieties of combination of the feldspar with the tourmaline: in the first, the tourmaline has imposed its form upon the feldspar; in the second, each has influenced the other.

1.



There are two types of the first variety. Fig. 1 shows the tourmaline with a solid termination, and the enclosed feldspar pierced with small tourmaline prisms which descend from the solid end with their vertical axes parallel to that of the enclosing shell. These shafts, as well as the shell, decrease in thickness as they recede from the head. The tourmaline has thus a pyramidal cavity filled with feldspar; and in one instance this cavity is terminated by a face of -1. The shell is absent in places, with the feldspar apparently crystallized according to the prismatic planes of the tourmaline. In figure 1, the dark and light parts represent tourmaline and feldspar respectively;

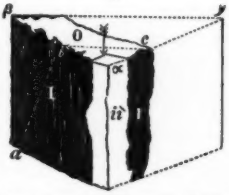
1*a*. 1*b*. 1*c*.

figs. 1*a*, 1*b*, 1*c* are sections of the same crystal (fig. 1) parallel to the basal plane of the tourmaline, and taken at the points indicated by the letters *a*, *b*, *c*; they show the gradual disappearance downward of the tourmaline.

In the second type the rhombohedral faces, as well as those of the prism, are shells, and the tourmaline does not seem to decrease in thickness with its distance from the termination. After removing the shell from these combinations the enclosed feldspar has been obtained having the form of a distorted tourmaline crystal.

Under this second variety there are cases in which the tourmaline prism encloses the feldspar and has no terminations. The feldspar often replaces the tourmaline, as is seen in Fig. 2, where the angles between the prismatic faces are distorted; one face is found parallel to a cleavage plane, usually $i\text{-}\grave{i}$ of the orthoclase. In the figure the angles $\alpha\beta\gamma$ and $\beta\alpha\gamma$ are respectively 45° and 90°; the tourmaline prism is perpendicular to the basal plane of the feldspar; the edge toward the eye has been replaced by the orthoclase, while the tourmaline makes a re-entering angle behind it at the arrow-point; the plane *abc* is the cleavage plane parallel to *I* of the feldspar. These prisms are readily separated from their feldspathic matrix, and cleave parallel to its basal plane; each section thus given shows an uneven shell of tourmaline that in places entirely disappears. In all the cases mentioned the cleavage of the enclosed feldspar is parallel to that outside.

2.



3.

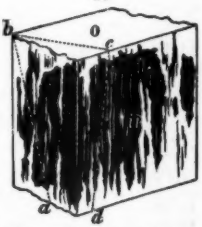


Fig. 3 shows where the tourmaline has been deposited on a cleavage face of the feldspar, and covers about one square inch; the vertical edge toward the observer is rounded, and presents no measurable angle; the plane *abcd* is a cleavage plane parallel to $i\text{-}\grave{i}$ of the feldspar. In one instance the feldspar inside the tourmaline encloses another shell of tourmaline filled with orthoclase.

The few specimens thus far obtained seem to point to simultaneous crystallization. There is no law governing the relations in position of the two minerals; for, in the same mass of matrix, there are crystals of tourmaline whose vertical axes make with the basal plane of the orthoclase angles varying from 0° to 90°. Some of these enclose the feldspar in prisms whose angles correspond exactly with those of theory, while others are distorted and are found between the cleavage planes of the feldspar. A more extended study of these interesting forms, afforded by a greater number of specimens, may throw more light on this singular combination.

Philadelphia, Dec. 28, 1875.

Art. XXXIV.—*The Conglomerate Series of West Virginia;* by William M. Fontaine.

In the May and June numbers of this Journal for 1874, I gave some account of the strata which, on New River, West Virginia, underlie the massive sandstone exposed at the Falls of the Kanawha. This account was necessarily imperfect, since at the point examined the base of the series was not exposed, and the exposures were very unfavorable for a detailed examination.

During the past summer I revisited this field, and made further examinations, at points more to the east, with such success that I am now able to present a detailed section of this field. Since the white sandstone of the Falls is the equivalent of what is everywhere called "the Conglomerate" of the Coal Measures, it might to some seem more fitting to call the rocks in question "Sub-conglomerate," or "Lower Carboniferous." In West Virginia, the strata which occupy the interval between the floor of the productive coals and the Devonian, are so greatly expanded, and so much diversified, that these terms are not definite enough to distinguish them. Besides, this New River system occupies precisely the horizon which is elsewhere commonly filled by conglomeritic sandstone alone, lying, as it does, between the red shales of the Umbral, and the lower productive coals. For these reasons I prefer to use the name "Conglomerate Series" for it. For like reasons it will be necessary to retain the names "Vespertine" and "Umbral," of the first Pennsylvania Survey, in describing rocks equivalent to those bearing these titles in the above named survey. A single instance will show this necessity. The system about to be described contains important coals. We find also far below them, in the Vespertine of Montgomery County, Virginia, near the White Sulphur, West Virginia, and elsewhere, well developed coals. To call these Sub-conglomerate or Lower Carboniferous coals would fail to distinguish them.

In my second visit to this region I made a re-examination of the strata at Sewell Station, the point at which most of the facts given in my first paper were obtained. In this last visit I found the strata quite well disclosed in the cuttings of the "Incline," made since my previous inspection. I also made a careful and detailed examination of the same strata at Quinnimont, a point on the Chesapeake and Ohio Railroad, distant by railroad twenty-one miles to the east of Sewell Station, but about ten miles by air line.

While the base of the series is not exposed at Sewell Station, yet, owing to the fact that the westerly dip is more rapid than

the fall of the river, even the underlying Umbral red shales are fully disclosed, and the entire series in question is contained in the lofty hills at Quinnimont, while in their summits they still retain a small remnant of the lower productive coals, with one and sometimes two coal beds.

While making my examination at Quinnimont I received valuable aid from Mr. S. F. Morris, C. E, and I take this opportunity to make my acknowledgments to him. Mr. Morris had, by levelling, determined the height of many points, and examined the character of the strata around Quinnimont, in behalf of the company owning the furnace and coal mine at that point. The data which he kindly put at my disposal were of great assistance in checking my own observations.

During the same summer I also made an examination of the country to the east of the Quinnimont, especially that portion in the vicinity of the White Sulphur Springs, Greenbrier County. It will perhaps be well to give here some of the facts thus obtained, bearing on the general geology of the region, in order more clearly to define the relations of the series to be described in this paper. In order to do this I will commence at the east and proceed west along the line of the Chesapeake and Ohio Railroad, whose general course is across the strike of all the strata underlying the rocks in question.

We may for a clearer exposition commence at Lewis Tunnel, a point six miles east of the White Sulphur. Here we find Vespertine strata which run in a narrow belt along the east face of the main Alleghany range, and contain the small coal beds, and plant-bearing shales, found near the Tunnel. The main range and the country westward for twelve miles is occupied by highly disturbed Devonian strata, mainly Hamilton, Portage, and Chemung, with probably the Catskill group. In the center of this belt the Springs are situated. Six miles west of the Springs, we find, on the east side of a small creek, highly contorted Devonian strata, and on the west side within 100 yards, the upper portion of the Vespertine, dipping gently eastward toward the contorted Devonian. Just above the Vespertine, in the hill across this stream, the base of the Umbral or Lewisburg limestone may be seen. The contortions and other evidences of great disturbance which follow us from the east up to this point now cease, and throughout the wide belt of country lying between this point and the Ohio River, the strata undulate more and more gently, until before Quinnimont is reached the rolls cease to reverse the dip, but serve to keep the strata longer at the surface than they would otherwise remain in that position.

This sudden change in structure is not found here alone, although it seems to be more marked here than elsewhere. It

may be traced far to the southwest, and probably to the north, and is to be explained by the existence of a fault, apparently the most westerly of the system affecting the Appalachian Region in this quarter. The development of this fault seems to have, in a great measure, relieved the strata lying to the west, from the disturbing force which so highly affected them on the east and it is not necessary to suppose a gradual dying away to the westward of the lateral thrust from the east. The conditions seem to imply a certain amount of unconformability between the Devonian and Vespertine, which is not incompatible with other facts observed here.

Proceeding westward along the line of the railroad from the fault, the gentle rise of the Vespertine to the west brings into view its middle or coal-bearing portion, here also containing small coal seams. This is in the vicinity of the bridge over Greenbrier River, and explains the presence near this stream, of the coal seam mentioned by Prof. Wm. B. Rogers in his reports. The Vespertine as it crosses the stream passes into a low anticlinal, which, west of the river, finally brings down the Umbral limestone to the level of the railroad. This position it maintains for a long distance, as far as Great Bend Tunnel, near the mouth of the Hungert's Creek, Summers County, where it dips under the red rocks of the Umbral series, which in this district are greatly developed. The Umbral series seems to possess a threefold character, being at bottom blood-red shales and sandstones, in the middle, grayish, bluish and brownish sandstones and shales, mainly the former; and at top brownish sandstones, blood red and varigated shales. The shales throughout the series have the texture of marlites, and the sandstones, although chiefly argillaceous are sometimes highly siliceous, forming huge cliffs along the railroad, as seen near Richmond Falls. These three series, the Vespertine, the Umbral limestone, and Umbral shales and sandstone, thicken rapidly in proceeding from northeast to southwest. Prof. Rogers measured them in Greenbrier Mountain, Pocahontas County, a point about sixty miles northeast of Richmond Falls on New River, where the Umbral shales and sandstones are extensively exposed. With respect to the limestone I have no data for comparison but the indications are that on the railroad it is thicker than the measurement given by Prof. Rogers, viz: 822 feet. For the Umbral shales and sandstones in Pocahontas he gives a thickness of 1,310 feet. My estimates along the line of the railroad, which, however, have not the accuracy of measurements, give for this series a probable thickness of 1,450 feet in the vicinity of Richmond Falls, distributed as follows: 1. Lower red shales and sandstones, 320 feet; 2. Middle gray and greenish sandstones, 820 feet; 3. Upper red and variegated shales, 310 feet. If we com-

pare this series with the character of the Umbral in the vicinity of Blossburg, as given by H. D. Rogers, we find an almost identical distribution of similar strata. The upper portion of the Umbral continues to be shown up to a short distance west of Quinnimont, where the prevailing westerly dip takes it out of sight. These upper strata form the base of the hills around Quinnimont, contrasting strongly in all their physical features with the overlying conglomerate series. This latter, whose entire thickness lies above the level of the river at this point, gradually sinks as we pass west, down stream, being kept above water level for a long distance by several broad rolls. It finally passes out of sight two or three miles below Kanawha Falls, and is succeeded by the series of the Lower Productive coals in the Kanawha Region. In this latter series there is a four-feet bed of coal, about forty feet above the massive sandstone which closes the conglomerate series. This is the equivalent of coal B of Lesley. This bed still remains uneroded in the tops of some of the high hills around Quinnimont.

About two miles down the river, on the Raleigh County side, Piney river empties into New River. A well graded road from the mouth of this stream, passes over the outcropping edges of the entire conglomerate series, and the numerous cuttings made in grading afford excellent exposures of almost every member of the series throughout its entire thickness. My section was made along this road. It was verified by a second section taken at Quinnimont by another road, which also passed over the entire series. These two sections were compared with observations made at Sewell Station, and with measurements made by Mr. Morris. The data in the cases in the section, marked as not seen, are given on the authority of this gentleman. The dip is northwest about fifty feet to the mile.

Section from the mouth of Piney River, Raleigh County.

21. Upper conglomerate, 150–200 feet.
20. Black slate, with thin coal partings, coal not seen, 10 feet.
19. Olive gray sandstones and shales, 100 feet.
18. Dark blue slates and sandstones, 80 feet.
17. Quinnimont coal seam, or coal No. 9, consisting of semi-bituminous coal, 4 feet; fire clay, 2½ feet; and at bottom, splint coal, 14 inches, =8 feet.
16. A thick mass of rocks not fully exposed, which may be divided as follows: 16 *e*, olive gray shaly sandstones, 40 feet; 16 *d*, coal 8, not seen, given as 20 inches thick; 16 *c*, bluish sandy shales, 60 feet; 16 *b*, coal 7, not seen, given as 2 feet thick; 16 *a*, gray sandstone, 50 feet. Total, 154 feet.
15. Fire clay and a 12-inch outcrop of coal, seen imperfectly exposed, given as 2-4½ feet of imperfect splint coal, coal No. 6, =2-4½ feet.

14. Coal system; at bottom interstratifications of coal and slate, with one seam one foot thick; (coal No. 5), and on top, flags passing into firm sandstones. Good plant impressions occur here. Thickness, 80 feet.
13. Olive marlites, 40 feet.
12. Massive firm gray sandstones, 50 feet.
11. Coal No. 4, not fully exposed, given as 2½ feet thick.
10. Firm gray flags and sandstones, 90 feet.
9. Coal system, coal No. 3; at bottom interstratifications of thin coals and strata; on top, shales, flags and sandstones, 80 feet.
8. Gray sandstones, 75 feet.
7. Ferruginous limestone, 2 feet.
6. Variegated marlites, 40 feet.
5. Bright red shales and marlites, 30 feet.
4. Coal system, coal No. 2, consisting of coal 8 inches, slate 2½ feet, coal 8 inches, sandstone 8 feet, and at bottom coal and slate 1 foot; total =13 feet.
3. Olive and reddish sandstones, passing below into olive marlites, 100 feet.
2. Black slate, not seen, said to contain 18 inches of coal, (coal No. 1), given as 11 feet thick.
1. Lower conglomerate, 80 feet. Total =1,197 feet.

Under the lower conglomerate is found a transition series, of which the following is a section determined mainly at Quinnimont, where the strata are more fully exposed.

Transition Series at Quinnimont.

2. Black fissile slates and shales, 20 feet.
1. Thinly laminated gray flags and calcareous shales, with drifted leaves of Lepidodendra near the base; and near the top having numerous impressions of marine shells, while at the top it passes into carbonaceous shales with strings of coal, leaves of Lepidodendra and other impressions too much obscured for determination, 50 feet. Total =70.

To complete the section of the strata exposed in the vicinity of Quinnimont, I give below a section of so much of the Umbral series as is to be seen there.

Section of the Umbral Series at Quinnimont.

3. Variegated marlites with some nodular limestone, 70 feet.
2. Gray calcareous sandstone, 20 feet.
1. Bright red shales, seen 50 feet. Total =140 feet.

Some of the above mentioned strata merit a more particular description, which I will now give.

I make in this place no further mention of the remnant of the Lower Productive coals found in this vicinity, but refer to my former paper, where some account was given of their

character as found in the Kanawha Valley. It is not known how much farther east they extend, but it cannot be to any considerable distance. No. 21 of the conglomerate series is the only persistent member. As it is found everywhere throughout the Appalachian Coal Field, being in many places the sole representative of the series, and as it is always at a uniform distance below the lowest workable coal-seam of the Lower Productive coal it would seem to been titled to be called, as it has been, "The Conglomerate of the Coal Measures." In Raleigh County, and along New River, it is usually a coarse white sandstone, with some conglomeritic portions in its middle and upper parts. In its lower portions it is more flaggy and argillaceous. It varies in thickness from 150 to 200 feet. In the section I have in my summation taken it at the lower figure.

No. 20, near Piney River, shows at its outcrop only black slate. It has been opened near Quinnimont, and is said there to contain thin strings of coal. Nos. 20, 19, and 18, have no features of special interest.

No. 17. This is the coal-seam which is worked extensively at Quinnimont, where it is coked and used in the furnace at that place. It is the most persistent and best developed seam of the series, being easily recognized everywhere in this region by its peculiar structure. From the flaggy sandstones over this bed at Sewell Station were obtained the plants of Devonian type mentioned in my former paper. At Quinnimont I could find none of these, and it is there remarkably free from plant-impressions of all kinds. In Raleigh also it showed no plants. At Sewell Station, this seam was at first opened for the purpose of working it, but was soon abandoned, owing to an apparent thinning out which was in fact caused by a slide.

No. 16, on the Raleigh road, was not fully exposed owing to slides, which also obscure its outcrop at Quinnimont. It presents the subdivisions founded on the character of the sandstones given in the section, but the coal beds are given on the authority of Mr. Morris, and others, who claim to have opened them. I have no doubt of their existence, for at Quinnimont the black slate accompanying 16 *d*, or Coal No. 8, was seen.

No. 15 was only partially exposed at its outcrop on the Raleigh road. Next to the Quinnimont seam, it appears to me to be the most promising seam of the field. The fire-clay is of fine texture, and sharply distinct from the coal, features not usually seen in the coals of this series.

No. 14, (Coal 5.) This presents in a marked manner a feature very common in this field. The coal at its base exists in the form of numerous interstratifications of coal in thin partings, and black carbonaceous shales; the whole being topped by strata which become more and more siliceous, and firmer as they ascend. There is enough carbon diffused through the

base of this mass to make an important bed of coal, were it collected in one mass. The condition of things here shown indicates that there was no deficiency of vegetable matter, but that the alterations of level were too rapid to permit a great accumulation of coal in one mass. The same features to a greater or less extent are shown in every coal bed of the series, and it is safe to say that the instability alone of the surface, prevented the accumulation, in this series, of coal beds as thick as those found in the more productive series which lies above it. Many good plant impressions occur here.

No. 11, from its outcrop, seems to be a promising bed of coal. Its thickness was not fully disclosed. Mr. Morris gives it as two and one-half feet thick. It shows at its base Stigmaria rootlets.

No. 10, stands out in high cliffs. Some of the other sandstones of the series also present firm perpendicular outcrops.

No. 9, is well exposed on the road in a high cliff. It presents the same features as number fourteen, even more strikingly. Numerous thin seams of coal, intermixed with carbonaceous shale, some of them three or four inches thick, form the lower portion for a space of seven feet. Vegetable matter in the form of films of coal, and impregnations of the sandstones and shales, occur to the height of thirty feet. Only a few Lepidodendron leaves were found here.

No. 8, is a massive and siliceous sandstone, forming high cliffs, and resembling to some extent No. 21.

Nos. 6 and 5, are interesting for the recurrence here, in the middle of this coal-bearing series, of the same conditions which prevailed in the formation of the upper part of the Umbral series. These two strata are most strikingly like the red and variegated marlites and shales, found in that portion of the Umbral, and might easily be mistaken for them.

No. 4, is well exposed on the Raleigh road. No plants were found in it.

No. 2, is not exposed anywhere so far as I have seen. The interval occupied by it, lies between the massive rock, No. 1, and the crumbling strata of No. 3, which are especially prone to slide down over the precipitous cliffs formed by No. 1. Hence at all the places examined by me, this portion was buried under a mass which had come down from above. Its character is given on the authority of Mr. Morris.

No. 1. This member of the series I consider to be the base of the conglomerate series. It is one of the most prominent features in the hills, standing out as it does, not far above their bases, in immense precipitous ledges. It forms the first stratum, which indicates a decided change from marine to terrestrial conditions. It is much nearer a true conglomerate than No. 21, for many of the layers contain pebbles, a half inch in diameter.

It is usually a coarse, open-grained, purely siliceous sandstone, lying in very thick beds. Near the bottom it is brownish in color, but above it is white, having many ferruginous stains. In many parts of this sandstone, particles of carbonaceous matter, in the condition of charcoal, are seen, produced from drifted fragments of trunks and limbs of trees. This condition of the vegetable matter is no doubt due to the ready escape of the bituminous matter from the porous sandstone. Sometimes pretty large angular fragments of the brown sandstones of the Umbral are found associated with these fragments of trees, and in some cases the pebbles of the conglomerate portions are of limestone. This rock is no doubt the heavy sandstone mentioned by Professor Rogers as found some distance to the east of this point, forming the summit of Little Sewell Mountain.

Underlying this rock is found a series of beds which are evidently the products of a period of transition. They are well exposed near Quinnimont, and exhibit some interesting features. No. 1 of this series is a thinly-laminated, argillaceous, gray sandstone in its lower part, but becomes more and more calcareous toward its upper portion, where numerous impressions of shells are found, a list of which will be given farther on. At its summit, which is not seen at Quinnimont, but is well exposed on the Raleigh road, there is a good deal of vegetable matter mixed with the shale, and which is the product of plants which have grown on the spot. This is the lowest indication of an attempt at coal formation, seen in this region. From the indications, there is little doubt that in some places this horizon may show a little coal. Professor Rogers mentions that near the top of Little Sewell, and immediately over the red shales of the Umbral, he saw a small coal-bed. It is no doubt the stratum now described. The other strata given in the sections above present no points of interest.

From this account of the coal-bearing series in question it will be seen that it occupies the horizon of the so-called "Coal-measures Conglomerate," and it would seem to be simply a greatly expanded portion of this widely extended formation. Lying between two huge plates of massive sandstone, either of which has equal claims to the title of conglomerate, the name which I have given it seems justified.

Almost no exploration has been made in the country to the east of Quinnimont, and hence the limits in that direction of this series cannot be given. That it does extend farther east is known. Since my inspection last summer, I have been informed that a five-foot bed of coal is found near Hinton, 800 feet above the level of the river. Hinton is near the mouth of Greenbrier river, about fifteen miles farther east than Quinnimont, measured in an air-line across the strike of the strata.

To the southeast and south, it is found in the counties of

Wise, Russell, and Tazewell, as may be seen from the account of these counties given by Professor Lesley, in his paper read before the Am. Phil. Soc., April 21, 1871. Professor Lesley shows that under the so-called "Sheep Rock" in Wise county, about 700 feet of coal-bearing rocks are disclosed, with the base not shown. The "Sheep Rock" is No. 21 of the Piney River section. In this space two coal beds are to be seen; one, a six-foot bed, lies at the very base of the hills, and the other, a two-foot bed, is a short distance above it. A similar formation exists in Russell and Tazewell counties. These coals are not to be confounded with the beds seen in Montgomery county, for the latter are found in the Vespertine strata, and are of the same age with those near the White Sulphur in Greenbrier county. The basin, in which these conglomerate coals were formed, evidently extended still farther east than the counties described in Professor Lesley's paper, as the considerable development of this series in them shows. But in the more easterly extension of the field, the number of seams have diminished, especially in the upper part. On New River in Raleigh county the most important coals are found within 700 feet below the upper ledge.

As we proceed northward, along the eastern outcrop of the series, it has been more extensively affected by erosion, and has been swept off from the greater part of Monroe and Greenbrier counties, these being occupied mainly by the Umbral shales and limestone. Professor Rogers mentions finding at the top of Greenbrier Mountain, in the northeast part of the county of that name, a massive sandstone resembling the conglomerate. This is no doubt a remnant of the series. North of this point, in Rich Mountain, in Randolph county, the entire series is presented, capping the mountain, according to Dr. Stevenson. But here it has undergone an important modification, from the loss of the shaly central portion, and the almost entire disappearance of the coals.

[To be continued.]

ART. XXXV.—*Results of Experiments on the Set of bars of Wood, Iron, and Steel, after a Transverse Stress;* by WM. A. NORTON, Professor of Civil Engineering in Yale College.

AT intervals, during the last two years, I have carried on a systematic series of experiments, with the view of determining the laws of the set of materials resulting from a transverse stress under varied circumstances. The experiments were made with the testing machine which I devised several years since, for the purpose of experimenting on the deflection of bars under a transverse stress. A detailed description of this

machine is given in the Proceedings of the American Association for the Advancement of Science, Eighteenth Meeting, Aug., 1869, (p. 48). The depressions of the middle of the bar experimented on,—while under a transverse stress, or remaining after the stress has been withdrawn—are measured by it to within $\frac{1}{1000}$ of an inch. The experiments on set have been fully discussed in two papers read before the National Academy of Sciences, Washington, (April, 1874 and April, 1875). The first paper set forth the results of the experiments on bars of wood, and contained a detailed account of the course of experiments instituted for the purpose of detecting instrumental errors, and of the precautions taken to reduce the incidental errors, from variations of temperature and other causes, to a minimum. The second paper discussed the experiments on the set of bars of wrought iron and steel; which gave results generally similar, under corresponding circumstances, to those obtained with wood. I propose, in the present communication, to give a succinct statement of the general conclusions that follow from the whole discussion.

The experimental investigation was prosecuted under three general heads:

I. Sets from momentary strains.

II. Sets from prolonged strains.

III. Duration of set; and variation of set with interval of time elapsed after the withdrawal of the stress.

Each of these embraced several special topics of inquiry. The bars used in most of the experiments consisted of one of white pine, 3 in. by 3 in and 4 ft. long; another of wrought iron, $\frac{1}{4}$ in. wide, 1 in. deep, and 4 ft. long; and a third of steel of the same dimensions. The discussion of the entire series of experiments has brought out the following results, as alike applicable to bars of wrought iron, steel, and white pine.

1. The immediate set,—that is, the residual deflection which obtains immediately after the transverse stress is withdrawn,—increases in nearly the same proportion as the stress applied; until this exceeds a certain amount, beyond which the set increases according to a more rapid law than that of proportionality to the strains. It is to be understood here that the varying strains are applied at considerable intervals of time.

2. The immediate set augments with the duration of the stress, up to a certain interval of time. In the experiments with white pine, the duration of strain which gave the maximum immediate set, varied, with the strain, from ten minutes to one hour. The immediate set resulting from a prolonged strain, was found to be from five to nine times as great as that which succeeded a momentary strain.

3. The residual depression below the original line of the bar,

is greater if the stress is reached by a series of increasing weights than if the full stress is directly applied.

4. When the same strain is repeated on the same bar, after a short interval of time, the set first obtained is not augmented, unless the load applied exceeds a certain amount, varying with the material and dimensions of the bar. With loads greater than this limit each repetition of the load augments the total set. The amount of the increase varies with the interval of time since the previous application of the load and the number of previous applications.

5. The set, or residual depression of the middle of the bar, experiences marked variations as the interval of time subsequent to the removal of the stress increases. When the immediate set is less than about 0·0005 in. it passes off in a few minutes (10 m. or less). When it is greater than this it habitually varies as follows: it invariably decreases for a short interval of time, and then ordinarily increases for a longer interval, with moderate fluctuations. The period of decrease varies from about 5 m. to 20 m.; and is the longer in those instances in which the stress is prolonged. The subsequent increased set, or augmented depression of the line of the bar, may attain in less than an hour to an amount even greater than the set observed immediately after the stress is withdrawn. In some of the experiments the depression increased until it came to be about double that first observed. The proportionate increase of set is usually, however, much less than this. This increase of set is eventually succeeded by another decrease. These remarkable fluctuations observed in the line of the bar were more conspicuous in the experiments with white pine, than in those with iron and steel. The difference was, however, only in degree. Under similar conditions the general character of the fluctuations was the same whichever material was used. The fluctuations observed with the bars of iron and steel, as well as with the wooden bar, far exceeded any errors to which the observations were liable. They were also much too slow, and too prolonged, to be regarded as simple vibrations of the bar, consequent on the removal of the downward pressure.

6. Abnormal variations from the general law of variation of the set just noticed, may occur under especial circumstances. Such deviations were observed after the bar had been subjected to repeated strains from day to day. Under these circumstances the bar may be in such an abnormal condition that the set observed immediately after the stress is withdrawn may pass off rapidly, and the line of the bar may even rise considerably above the position held when the stress was applied—though not above its original line some days previously, before any strain was applied.

7. When the load, or stress at the middle of the bar, exceeds a certain amount, the set resulting from one or more applications of the load on any one day is not only still discernible on the following day, but the actual result may be that the middle of the bar may be lower than at the close of the observations on the previous day. Such effects were observed, in the experiments with white pine, when the load was sufficient to produce a longitudinal strain on the upper or lower fibers of 500 lbs. per square inch; and in the experiments with the steel bar, resting edgewise on its supports, when the strain on the outer fibers amounted to 1500 lbs. per square inch.

8. Repeated applications of the same load, from day to day, are attended with an indefinite augmentation of the residual depression of the middle of the bar, if the load exceeds a certain amount. When a smaller load is similarly applied, the set attains after a few days to a maximum, and subsequently subsides more or less. The load answering to the critical point here referred to, is obviously the maximum safe value for a variable load that can be applied, with an indefinite number of repetitions, to the bar. In the case of a white pine stick (3 in. by 3 in., and 4 ft. long) the experiments show it to be less than $\frac{1}{4}$ the theoretical breaking load. Under repeated applications of 500 lbs. (or about $\frac{1}{4}$ the theoretical breaking weight) the set steadily increased from day to day—that is, the middle of the stick became more and more depressed—during the entire period (seven days) that the prolonged effects were noted. Under daily repetitions of a load equivalent to $\frac{1}{7\cdot5}$ the breaking weight, the depression increased for three days, and after another interval of three days the stick had recovered its original line. The depressions here referred to are those which obtained on the morning of each day just before the first application of the stress on that day.

9. In connection with the phenomena of set which have been signalized, it is important to note that during any interval in which a bar was kept under a transverse stress, the resulting deflection commonly experienced a continual variation. In general the deflection increased as the strain was prolonged. But the deflection of the steel bar in some instances diminished, under the prolonged strain. This unusual result was apparently dependent on some molecular condition of the bar, induced by previous strains. The comportment of the wrought iron bar, as regards varying deflection under a continual strain, was not particularly examined.

It is also noteworthy, in this connection, that the deflection resulting from any single stress was found to be more or less dependent on the previous strains to which the bar had been subjected. The wooden bar, when it had been exposed to a

cross strain not long before, was generally in a condition to suffer a greater deflection than it had before experienced under the same load. The same was true of the steel bar during several successive days of experiments with loads of 4 lbs. and 6 lbs.; but as the result of these repeated strains the bar came eventually to be in a condition in which each renewal of the stress gave, for the most part, a less and less deflection.

10. It is apparent from the foregoing experimental results, that every application of a transverse stress to a bar must induce some change in its molecular condition, which continues, with variations that may be either progressive or fluctuating, for a greater or less interval of time. The duration of sensible influence varies with the amount and duration of the stress. For the smaller strains it is but a few minutes; for the larger several days. The prolonged influence of strains applied from day to day to a bar, was apparent from the fact that the same stress did not on different days produce either the same deflection or the same set. It was strikingly shown in the experiments with the steel bar by causing the bar, to which loads had been repeatedly applied for several previous days, to rest on its opposite side, and comparing the deflection and set with those obtained immediately before the reversal. It was found that the deflection produced by $18\frac{1}{2}$ pounds was $\frac{1}{10}$ greater than the deflection produced by the same weight just before the reversal; and the set obtained was now many times greater than before. The deflection also now increased with a prolongation of the strain, whereas it before decreased. Also the set now increased for a considerable interval of time after the withdrawal of the strain, whereas it before decreased.

11. There was no discernible limit of elasticity, revealed by the experiments, with either wood, iron, or steel. A perceptible set obtained, with each material, immediately after the stress was removed, however small its amount, until the set fell below the lowest possible determination of which the apparatus was capable (viz: $\frac{1}{10}\frac{1}{0}\frac{1}{0}\frac{1}{0}$ of an inch, as the experiments were ordinarily conducted.) To test the question still farther, the delicacy of the measuring apparatus was largely increased, by the adaptation of a device for magnifying the movements to be observed: and it was found that the least perceptible immediate set was still limited only by the capability of detecting, with the apparatus, minute displacements.

If we take for the limit of elasticity the condition of things at which a *permanent* set is obtained, the case is different. Thus it was found that the set which subsisted after the pine stick (3 in. by 3 in. and 4 ft. long), had been loaded at its middle with 200 pounds ($\frac{1}{10}$ the theoretical breaking weight), eventually passed off entirely. This was the case whether the stress was momentary or prolonged, and whether it was applied but once

or repeatedly. But with a load of 500 lbs. a permanent set was obtained, as the result of a single application of the stress; and repetitions of the stress were attended with a continual increase in the depression of the middle of the bar. It may accordingly be affirmed that a practical limit of elasticity exists, but not a theoretical one.

12. If a bar, on the withdrawal of a transverse stress, fails to recover its original line of position, or, technically speaking, has a set, it is plain that its integrant molecules have not returned precisely to their original positions, and that the distances between contiguous molecules have either increased or diminished —increased in the line of the longitudinal fibers that have experienced a tensile strain, and decreased in the line of those which have experienced a compressive strain. Now we have seen that, as the result of a series of increasing transverse stresses, the set increases continuously with the stress, from the lowest amount capable of detection with the measuring apparatus employed. We must therefore conclude that, after the application of a series of increasing strains, in which the molecules are relatively displaced by minute fractions of their intervening distances, they take up, when the strain is removed, a series of new positions of equilibrium, differing by excessively minute degrees from those previously occupied. We may draw the same conclusion from the experiments on the set produced by a series of direct tensile and compressive stresses, made by Hodgkinson, Chevandier and Wertheim, and other experimenters. This general conclusion, to which experiments on set, under every variety of strain, conduct, leads to the inevitable inference that *the effective forces exerted by the molecules on one another have suffered some change of intensity, in consequence of the stress applied to the bar under experiment.* Viewing the residual displacement of the molecules, in their relative positions, as a mechanical problem, we are constrained to regard the effective molecular forces, that take effect at a given distance, as having acquired a different intensity. We have confirmatory evidence of this induced molecular condition of the bar in the fact that all the diverse effects, which may ensue on subsequent applications of a transverse stress, are found to be either less or greater than those previously observed under similar conditions.

13. The fluctuations that have been noticed as occurring in the set with the lapse of time, reveal the fact that the change in the intensities of the effective molecular forces, which results from the temporary application of the stress, is not permanent but fluctuating; and may, according to the amount of the stress applied, rapidly pass off, or, after a partial collapse, be slowly recovered again. It should be observed, however, that the curious fact of the increase of set which ordinarily succeeds the first sudden fall, may be in part attributable to the gradual

propagation inward of the greater disturbed condition of the molecules of the upper and lower fibers.

14. The general correspondence in the phenomena of set and altered deflection, that obtain with different materials altogether precludes the idea that they may result, either wholly or in a considerable degree, from irregular strains subsisting in certain parts of the bar before the stress is applied, and which are more or less modified by the stress; as some persons have conjectured. The change that supervenes must be a general one, or one in which all the molecules participate, though in diverse degrees according to the amount of molecular displacement. The especial character of the change, for each individual molecule, must depend upon the kind of strain to which the molecule is exposed, whether tensile, compressive, or shearing; and not on the nature of the material subjected to strain.

15. If, as experiment has established, when the distance between two contiguous molecules has been forcibly altered, the molecules, when again left to their mutual actions, no longer exert, at the same distance, effective actions of the same intensity as before, it is apparent that *the molecules in the act of displacement have experienced some change, either in their dimensions, or in their internal mechanical condition.* This change must result from the change that took place in the mutual action of the molecules when they were urged nearer to each other, or separated to greater distances. It must be experienced by the ultimate molecule, whether this be indentical with the integrant molecule or not—that is whether we regard the integrant molecule as a single ultimate molecule, or as a group of ultimate molecules. For it is plain that a group of ultimate molecules could not undergo an internal change, that abides after all external actions have ceased, unless its constituent molecules have suffered a change, by reason of which they no longer act upon one another with the same intensities of force as before.

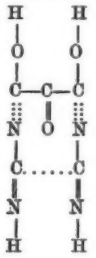
It is well known that with Physicists the "chemical atom" has come to be replaced by the "ultimate molecule." Of the probable physical constitution of the ultimate molecule different conceptions have been formed. To those Physicists who regard it as made up of a limited number of precisely similar atoms, endued with unvarying forces—of attraction at certain distances, and repulsion at other distances—I leave it to reconcile this conception with the legitimate inference to be drawn from experimental results, that the ultimate molecule is liable to a change of mechanical or physical condition, with every slight displacement it may experience—a change which subsists after the constraining cause of the displacement has ceased to act; and may, under different conditions, either be permanent, or gradually subside with fluctuations.

ART. XXXVI.—*On the constitutional formulæ of Urea, Uric Acid, and their derivatives;* by Professor J. W. MALLET, University of Virginia.

[Continued from page 194.]

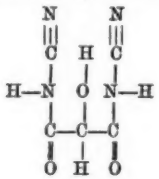
FOR uric acid itself, $C_5H_4N_4O_3$, the di-ureide of mesoxalic acid, I would propose the formula (No. 1).

1. Uric acid (di-basic).

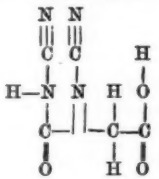


Nos. 2 to 9 show, for the sake of comparison, some of the numerous formulæ which have of late years been given for this important substance.

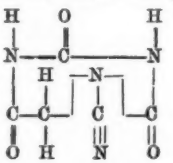
2. Uric acid, Baeyer* and Kolbe.†



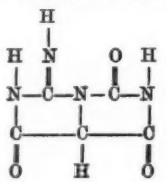
3. Uric acid, Hüfner.‡



4. Uric acid, Mulder.§

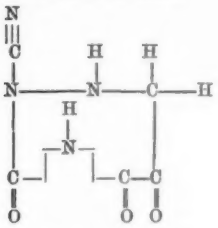


5. Uric acid, Erlenmeyer.‖

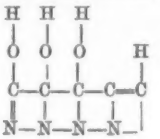


* Ann. der Chem., cxxvii, 235.
† Journ. für prakt. Chem., II, i, 134.
‡ Journ. für prakt. Chem., II, iii, 23.
§ Bericht. d. deutsch. chem. Gesellsch., vi, 1237.
‖ Münch. Akad. Ber., ii, 276.

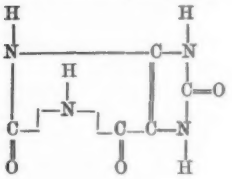
6. Uric acid, Strecker.*



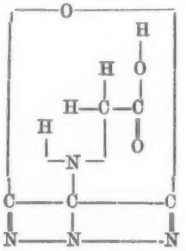
7. Uric acid, Gibbs.†



8. Uric acid, Medicus.‡



9. Uric acid, Drechsel.§



While each of these formulæ possesses advantages for the explanation of certain cases of decomposition and certain derived products, an attentive study will show, I think, that all are more or less defective as to accounting in the simplest way for the well known basicity of uric acid itself, bringing it into harmony with the general structure of non-nitrogenous organic acids, recognizing a close relationship to the 3-carbon series, and preserving as far as possible simplicity and symmetry in the supposed arrangement of the atoms.

In connection with the formula I propose it may be noticed: that it does account for uric acid being dibasic; that it derives it as directly as possible from a residue of the 3-carbon mesoxalic acid; that it explains simply most of the observed decompositions of the acid; that it perhaps affords a reason, in the direct linking together of the two urea residues as well as their attachment to the acid nucleus, for the comparative stability of uric acid; and that it also suggests a cause of the difficulty of reproducing this substance artificially, since in the attempt to form a salt of urea with a non-nitrogenous acid and then remove water the basic hydroxyl might be eliminated and the normal acid type destroyed, whereas this type is pre-

* Lehrb. d. org. Chem., 1868, 800. † This Journal, Nov., 1868, xlvi, 293.

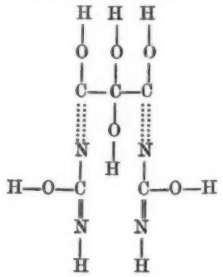
‡ Ann. der Chem., clxxv, 243; where most of the formulæ above quoted are reviewed.

§ Chem. Centralbl., 4 Aug., 1875, 493.

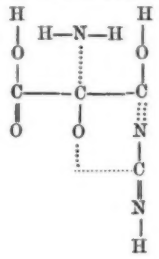
served by the different mode of attachment of the urea residues exhibited in the formula now put forward. The mode of production from uric acid of allantoine, alloxan, paraban, etc., will be seen by comparison of the preceding formulæ.

Probably uroxanic acid, $C_5H_8N_4O_6$, is represented by No. 1

1. Uroxanic acid (dibasic).



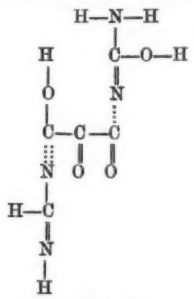
2. Oxonic acid (dibasic).



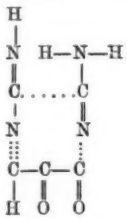
while the oxonic acid of Strecker and Medicus,* $C_4H_5N_3O_4$, produced under similar conditions, but showing by its containing only three atoms of nitrogen that it cannot include two complete urea residues like those of uric acid, may perhaps have the structure of No. 2.

The basicity of pseudo-uric acid, $C_5H_6N_4O_4$, may be explained by assuming its two urea residues differently attached, and in one of them an atom of hydrogen taking the place of hydroxyl—thus:

1. Pseudo-uric acid (monobasic).



2. Xanthine (weak base).

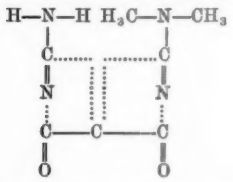


In xanthine, $C_5H_4N_4O_2$, whose empirical formula differs from that of uric acid only by an atom of oxygen, we have also two dissimilarly attached residues of urea, but the basic hydroxyl disappears altogether and with it the true acid character, while like urea itself xanthine is capable of uniting with metallic oxides as well as with acids.

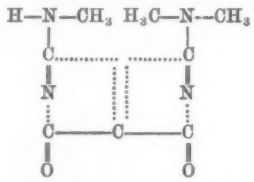
* Ann. der Chem., clxxv, 230.

This formula (No. 2) may serve to explain the fact that Strecker's di-methyl-xanthine is isomeric, not identical, with theobromine, $C_7H_8N_4O_2$, if we assume the latter to be as in No. 1, and caffeine, $C_8H_{10}N_4O_2$, as in No. 2,

1. Theobromine (weak base).



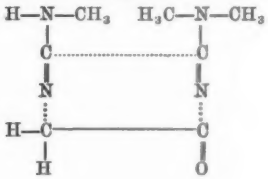
2. Caffeine (weak base).



both urea residues in each of these formulæ being similarly connected with the acid nucleus. The relation to di-methyl-paraban (cholestrophan) is obvious.

We may probably assume No. 1 as the formula of caffeidine, $C_7H_{12}N_4O$ (a stronger base than caffeine).

1. Caffeidine.



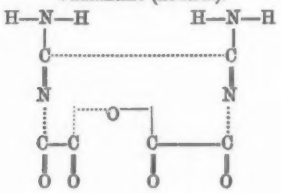
2. Hypoxanthine (sarcine)—(weak base).

Hypoxanthine, $C_5H_4N_4O$, exhibits the same general character with xanthine, and containing one atom less oxygen may be represented as in No. 2, above.

The above formulæ for hypoxanthine and xanthine accord well with the reported production by Strecker of the latter from the former by oxidation, and of a mixture of both bases from uric acid by reduction with sodium amalgam.

Passing to the compounds in which two acid residues are united with each other and at the same time with residues of urea, we may formulate oxalantine, $C_6H_4N_4O_5$, as follows:

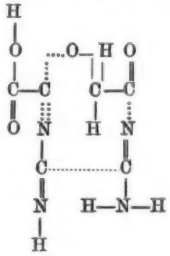
Oxalantine (neutral).



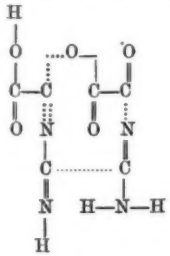
thus directly explaining the relation to paraban, and production therefrom by coalescence of two molecules with elimination of an atom of oxygen (or from a molecule each of oxaluric acid and paraban, with removal of an atom of oxygen and a molecule of water).

The union of hydantoine with allanturic acid, with separation of water, gives for allituric acid, $C_6H_6N_4O_4$, the formula No. 1,

1. Allituric acid (monobasic).



2. Leucoturic acid (monobasic).

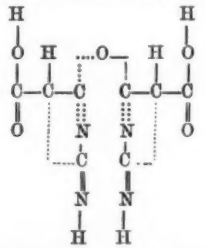


and the like union of paraban and allanturic acid leads to No. 2 for leucoturic acid of Schlieper, $C_6H_4N_4O_5$. This last formula explains the possibility at least of a difference between leucoturic acid and oxalantine, the identity of which does not seem clearly established.

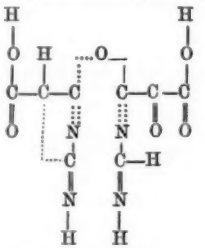
In each of the last two cases we have one ureic residue *inverted* as regards its mode of attachment to the acid nucleus when the coalescence takes place.

Two molecules of (dibasic) barbituric acid unite with separation of a molecule of water, giving rise to di-barbituric acid, $C_8H_6N_4O_6$, with unchanged basicity,

1. Di-barbituric acid (dibasic).



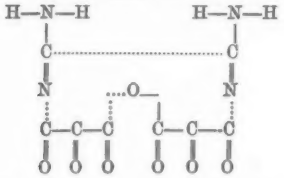
2. Hydurilic acid (dibasic).



while the union of a molecule of barbituric acid and one of dialuric acid, with *inversion* of the ureic residue of the latter and elimination of a molecule of water, gives us hydurilic acid, $C_8H_6N_4O_6$, (No. 2).

Alloxantine, $C_8H_4N_4O_7$, may be represented as

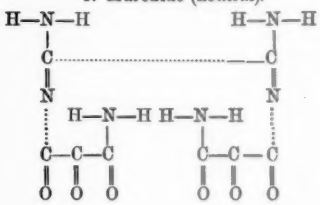
Alloxantine (neutral).



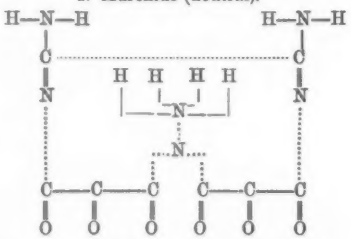
clearly exhibiting the analogy of this body to oxalantine, and its production in a similar way by the coalescence of two molecules of alloxan with separation of oxygen (or a molecule of alloxan and one of dialuric acid with the additional separation of water). The formula of Gibbs for alloxantine would seem to imply that it is a monobasic acid (or, according to the exact terms of his own definition,* tribasic).

Finally, it is difficult to suggest with confidence a formula for the problematical substance murexide, $C_8H_8N_6O_6$. If an amide character be admitted for it; and it does seem that evidence is still wanting to conclusively prove that it is an ammonium salt, especially in view of non-production of purpuric acid and the undoubted existence of isomeric iso-purpurates (and possibly other salts) which may have led to an undue assumption of identity of type between murexide and its metallic derivatives; we may perhaps assume this substance to have the formula No. 1,

1. Murexide (neutral).



2. Murexide (neutral).



in which the union of two molecules of dialuramide is effected, with elimination of hydrogen, by the linking together of the ureic carbon atoms. This view of the constitution of murexide (making it alloxantine-amide) obviously affords a simple explanation of its production from dialuramide by oxidation, from ammonium dialurate by heating, from alloxantine and alloxan by the action of ammonia, &c., and also suggests the probable

* Loc. cit.,—"cyanyl may be regarded as the acidifying term. Its quantity, therefore, determines the basicity of the acid."

ease with which isomeric changes may be brought about. If murexide be ammonium purpurate, the formula might perhaps be changed to the form in No. 2.

While the views above stated as to the structure of the numerous and interesting compounds derivable from urea and uric acid are liable to objection at sundry minor points, and in several instances other arrangements of the elements might be adopted without interference with the main idea, I believe that on the whole the constitutional formulæ set forth in this paper more nearly represent the present state of our knowledge of this group than any others which have been proposed, and especially possess the advantage of better explaining the chemical character or function of the substances referred to,* while at least equally well exhibiting the nature of the changes by which they are produced from each other.

University of Virginia, Nov. 4, 1875.

ART. XXXVII.—*On the Evidences of horizontal crushing in the formation of the Coast Range of California;* by JOSEPH LECONTE.

[Read before the National Academy of Sciences, November, 1875.]

IT will be remembered that in a former paper "On the formation of the greater inequalities of the earth's surface,"† I sustained the view that mountain ranges are formed wholly by a yielding of the crust of the earth along certain lines to horizontal pressure; not, however, a yielding by bending of the crust into a convex arch, filled and sustained by a liquid beneath, as has been supposed by some; but by a crushing or mashing together horizontally of the whole crust, with the formation of *close folds* and a thickening or swelling upward of the squeezed mass. I believe the structure of all mountain ranges, in which the stratification has not been obscured by metamorphism, would demonstrate this mode of formation.

I have long thought that the Coast Range in this vicinity is peculiarly adapted to exhibit in its structure the mode of its formation. It is destitute of granite axes, and it has been but little, in many places not at all, changed by metamorphism or overlaid by igneous ejections. A good section ought to clearly reveal its structure and its structure ought no less clearly to reveal the mode of its formation.

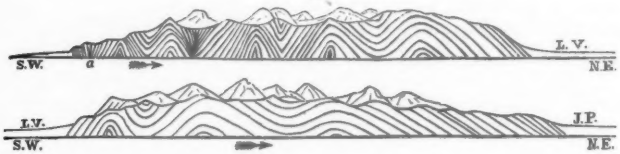
With this conviction, on the 5th of January last, in company

* The formulæ of Baeyer (in common with all those representing urea as carbamide) seem to fail in securing this advantage, although involving in part the same sort of view of the ureides as that above stated.

† This Journal, III, vol. iv, pp. 345 and 460.

with Mr. W. Jackson, a recent graduate of the university, and now a special student of mineralogy, I set off afoot, and walked very leisurely through the cut made by the Central Pacific Railroad from the plains adjoining the Bay of San Francisco through the mountains to the San Joaquin plains, a distance of about thirty miles, taking the angle and direction of the dip at every available point. The following diagram is a generalized section made from these observations, showing the structure of this range. The section is supposed to extend from southwest to northeast, i. e., at right angles to the direction of the chain, L. V. being Livermore Valley and J. P. San Joaquin Plains.

1.



The range where crossed by the railroad is divided into two sub-ranges separated by Livermore Valley. Both of these sub-ranges, it will be seen, are composed wholly of crumpled strata, those of the western sub-range or Contra Costa hills being crumpled in the most extraordinary manner.

The strata throughout the railroad cut are entirely unchanged and very distinct, and their dip may be taken with the greatest ease and certainty: but unfortunately they consist mostly of thin bedded shales and sandstones destitute of fossils, and so similar in appearance that identification of individual strata would be impossible without the most careful and detailed examination. Only in one place did I find any fossils, and these were easily identified as Miocene Tertiary. On account of the infinite number and the sameness of the strata I found it impossible to identify, and therefore I have not attempted in the diagram to trace the individual strata through the successive folds. But the general structure of the range is, I am sure, truly represented in the section.

A glance at the section shows that the southwestern sub-range or that next the bay is far the more complex. We have here at least five anticlinals with corresponding synclinals, all in a distance of about six miles in a straight line. The angles of dip vary from 40° to 90°, the average being about 65° to 70°. This would make the actual length of the folded strata two and a half to three times the horizontal distance through the mountain. Now it is not only impossible to conceive of the origin of such structure except by horizontal mashing, but the amount

of horizontal mashing must have been enormous. Estimating in the usual way, i. e., taking the present length of the folded strata as the original length of the strata when horizontal, there must have been fifteen to eighteen miles of original sea-bottom crushed into six miles, with *corresponding* upswelling of the whole mass.* I say estimating in the usual way: for the *real* breadth of the original sea-bottom was probably considerably less, since as I shall show hereafter, the strata themselves are probably lengthened in the direction of the dip.

Nor is this particular section an exaggeration of the general structure of this range. On the contrary it is far less complex here than elsewhere. A glance at Whitney's map of Central California will show that the range is small and low at this part. This exceptional lowness is due primarily to the less horizontal mashing, and therefore less *upswelling*, and therefore *less complexity* of folding, and therefore less metamorphism, and therefore less hardness of the rocks, and therefore also *greater erosion* of this part. Whitney has nowhere attempted to give a general section of this very complex range, but in fig. 1 on p. 14 of vol. i, of the Geological Survey, he gives a section of a small portion of the Contra Costa hills farther north, which shows much more crushing than any portion of the range cut by the railroad.

The diagram section is supposed to be made at right angles to the general trend of the range, i. e., northeast and southwest. The folds are of course represented as striking in the direction of the range, and dipping in the direction of the section. This is very decidedly the average direction of the folds; but there is considerable variation to either side of this average direction. This shows that the horizontal or folding pressure came from several slightly different directions, perhaps consecutively. The same is clearly shown in the external features, also, of this very complex range; for the sub-ranges and ridges of which it is composed trend in many different directions.

But there is another minuter structure which I have observed in some of the strata, both of the Contra Costa and the Mt. Diablo sub-ranges, which demonstrates, in the completest manner, the mashing together horizontally and the extension vertically, even of the *constituent particles* of the stratified sediments.

* In my paper "On the great Lava flood of the northwest," this Journal, III, vol. vii, p. 167 and seq., I have stated, pp. 170 and 180, that under all circumstances, whether the surface be *uppushed* by horizontal mashing of sediments or *upbuilt* by the *outsqueezing* of melted matter, the increase of height would be the same, being measured by the amount of horizontal crushing. Prof. E. W. Hilgard, in his paper "On Mallet's theory of vulcanicity," Am. Jour., vol. vii, p. 535, note on p. 544, takes exception to this statement. He does not see "on what ground a simple *uplifting*, can be considered the precise mechanical equivalent of an *upbuilding* by eruption of liquid rock." I take this occasion to say that Prof. H. has entirely mistaken the point. It is not a question of *mechanical*, but of *geometrical* equivalence—not an equivalence of *force*, but an equivalence of *magnitude*.

I have already stated that the mountain mass lying between the Bay of San Francisco and the San Joaquin plains is divided, by the Amador and Livermore valleys, into two sub-ranges; the Contra Costa, overlooking the Bay, and the Mt. Diablo,* overlooking the plains. Both Cretaceous and Tertiary strata are found in the latter, although their distribution has not yet been thoroughly worked out; but the former consists wholly of Tertiary, principally Miocene. In both these sub-ranges seams of lignite of good quality have been found. Those found in the Cretaceous of Mt. Diablo have proved of great value and are extensively worked; but as yet nothing but very thin unprofitable seams have been found in the Contra Costa.

Several months ago I was asked to examine the *croppings* of some thin seams of lignite near the town of Hayward, which had been opened to a depth of 100 to 150 feet. The coal-bearing strata dip nearly perpendicularly and strike in the general direction of the range. The place examined was on the lowest foot hills of the Contra Costa, corresponding in position to *a* in the section, fig. 1.

While examining the mode of occurrence of this lignite, my attention was drawn, by the intelligent Superintendent of this mine, to certain slabs of shale in immediate contact with the seam, which were literally covered with small rounded flattened masses looking somewhat like flattened pebbles. In fact he supposed them to be pebbles or shingle which had fallen into fissures between the perpendicular strata. Examination, however, quickly convinced me that they were not pebbles nor extraneous matter of any kind, *but clay pellets or nodules in the original sediment which had been flattened by strong pressure in the formation of the mountain range.* Here then, I saw at once a means of determining the amount of mashing to which the sedimentary strata had been subjected in the process of mountain-making. I immediately commenced closer examination.

The nodules were all greatly flattened and nearly all greatly elongated. Their shape therefore were mostly flattened ellipsoids, though some were flattened discs. The flattened ellipsoids were nearly all set on end between the strata, i. e., with their long diameters vertical, though some varied considerably from this position to one side or the other, and a few were nearly horizontal. They were found in close contact with the seam on both sides, and some in the seam itself; and in such numbers that they covered the surface of the strata. When small and disc-shaped, or not much elongated, the surface of the over-clay blackened by contact with the coal presented a

* The term Mt. Diablo range is usually used in a wider sense for the whole range on the east side of the Bay, as distinguished from the Santa Cruz Mountains lying on the west side. I use the term here in a narrower sense to distinguish a subdivision of this range.

striking resemblance to impressions of the trunks of Lepidodendrids. In other cases when greatly elongated they looked like parallel flattened root-fibers. The material of the nodules was similar to that of the containing clay, unless perhaps a little finer.

A few months afterward, March, 1875, in company with a party of students and graduates of the University, I examined the coal mines of Mt. Diablo, and there also observed, in the roof of the seam, flattened nodules of sandstone often surrounded with a thin layer of coaly matter; but the sandstone was coarse and the nodules were imperfect. Subsequently Mr. Christy, an assistant in the chemical laboratory, who is now engaged in an examination of the coals of this coast, visited the same mines more extensively and brought me some very fine specimens of flattened elongated nodules. In these also I am assured the long diameters were in the direction of the dip.

Now, there cannot be the slightest doubt that these nodules were once *clay pellets*, of all sizes, from that of swan shot to that of hazel nuts, which existed in, and on the surface of, the original clay sediments, having been taken up from finer deposits rolled along by gentle currents and deposited along with coarser material, precisely as we find at the present day; and further, that their present shape is due wholly to subsequent pressure, precisely as in the case of the greenish elliptical spots found in cleaved slates, and described by Prof. Tyndall:* and, therefore, finally, that by means of their shape and position, as in the case of the greenish spots, it is quite possible to determine the amount of mashing together in one direction and the extension or upswelling in another, which the sedimentary mass has suffered since its deposition.

I take the case of the Hayward seam as the simplest because the strata are vertical. Taking three *equal* rectangular diameters of the original unmashed pellets, one in the direction of pressure, i. e., horizontal and at right angles to the strata; another also horizontal but in the direction of the strike, and the third in the direction of the dip or vertical, it is evident that the first would be *shortened*, the third would be *elongated*, while the second would, on the average, be *unaffected*, since extension of this diameter in some places must be compensated by shortening in contiguous places right or left. We may assume, therefore, that the elongation vertically is strictly correlated with the mashing or shortening horizontally, and the one is a measure of the other. Now I found by careful measurement of a great number of these nodules that the shortest diameter bears to the longest the ratios of 1 : 3, 1 : 4, 1 : 6, 1 : 9, and even 1 : 12 and 1 : 14. I believe a fair average would be about 1 : 6 or

* Phil. Mag., xii, 35, 1856.

1 : 9. Now as this ratio is the result of *both* compression in one direction and extension in another, it follows that either the compression or the extension would be expressed by the square roots of these ratios. Therefore there has been a crushing together of every 2·5 to 3 parts into 1 and a corresponding extension in another direction of every 1 part into 2·5 to 3. But since the short diameters were horizontal and the long diameters vertical, it is evident that throughout the whole squeezed mass every 2½ to 3 feet were crushed together horizontally into one foot, and every foot of vertical thickness was increased or swelled up to two and a half or three feet. This seems to have taken place principally after, by folding, the strata had taken a vertical position. Therefore by the pressure *the strata were thinned and extended vertically.* No allowance has hitherto been made for this change in the estimates of the original thickness of folded strata.

There are several thoughts suggested by the above which I think worthy of mention.

1. The position of the nodules, sometimes on the surface of the coal seam, sometimes half buried, and sometimes wholly buried in the coaly matter, clearly proves that at the time when the nodules were first rolled along and deposited there, *the coaly matter was in the condition of very soft semi-liquid peat.*

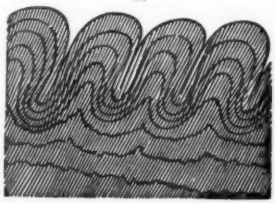
2. It is well known that slaty cleavage is produced by powerful pressure compressing the once plastic mass in one direction and extending it in another. The absence of slaty cleavage, under precisely these conditions, is evidently due, in the case under discussion, to the fact that the materials are unsuitable for the development of that structure, being *far too coarse.* If cleavage had been produced, however, *the planes of cleavage would have been parallel to the planes of stratification;* and, therefore, the structure would have been almost undistinguishable from, and liable to be mistaken for, a fine *lamination structure.*

Now, in many cases this parallelism actually occurs. On the foothills of the Sierra, especially about Snelling, Hornitos and Mariposa, are found fine clay slates beautifully fissile with their planes apparently perpendicular, but in reality dipping at a very high angle to the northeast, i. e., under the range. These are evidently true cleaved slates, and the very thin planes into which they easily split are true cleavage-planes and not lamination-planes. Yet I looked in vain for any stripe or other indication of stratification in any other direction. Also Whitney has shown (Geol. Surv., vol. i, p. 226), and I have myself observed, that these slates pass by insensible gradations into, and are even interstratified with, coarser materials, showing distinct stratification in the same direction, i. e., underdipping the

range at high angle. There can be no doubt, therefore, that *all* the strata of this foot-hill region, including the slates, underdip the range at high angle. Evidently, therefore, the cleavage planes of these slates are parallel to the stratification planes instead of cutting through them at high angle as is most common.

The diverse relation of the cleavage to the stratification planes I explain as follows: In a thick mass of very fine sediments mashed together horizontally it is evident that the surface and upper portions would first be thrown into one or more close folds by which the strata are brought into a nearly perpendicular position, and then these would be thinned and extended vertically by the pressure as already shown in the previous portion of this paper: but the *deeper portions* would be less and less folded, until, very deep, the folding would cease altogether and the mashing would be by *thickening* only and not by folding. I have rudely represented these facts in the diagram, fig. 2, in which the parallel, nearly vertical lines, represent the cleavage. In such a mass of horizontally squeezed fine sediments, therefore, the cleavage of the upper parts would be *parallel* with the strata while that of the lower parts would be *perpendicular* or nearly so to the strata. If, therefore, the upper parts only should be exposed by denudation we would have an example of cleavage parallel to the strata, and we might be in doubt whether to call the planes cleavage-planes or fine lamination-planes; but if greater denudation should expose the deeper portions we would have an example of cleavage-planes cutting through the lamination-planes at a high angle and therefore very distinct from them.

2.



3. It is evident from the above that in many cases the thickness of the strata as we now find them may be very different from that of the original sediments. In estimating the latter, therefore, we must make due allowance for the great thinning in some cases and thickening in others produced by pressure.

4. In my paper on the formation of the great features of the earth surface, already referred to, I have attributed mountain elevation to horizontal crushing. Prof. Dana,* however, thinks that, although the idea of plication is evidently included in my view, yet it ought to have a larger place than my words seem to give it—for the amount of elevation by plication is many times ("ten-fold") greater than by simple crushing.

* Am. Journ., III, v, 428.

Perhaps I ought to have been more explicit in my statement, but it seemed to me unnecessary, because on the assumption of a solid earth the amount of elevation would be the same, or nearly the same, whether, by the horizontal pressure, the strata be thrown into *closed folds* (as is most common in mountain chains) or only thickened without folding. If every two or three parts in horizontal extent of sediments be crushed into one part, there must be a corresponding thickening of the whole squeezed mass, and, therefore, a corresponding elevation of the surface, whether the strata be closely folded or only thickened without folding. In reality, doubtless, both occur in every case; close folding in the upper parts and thickening without folding in the deeper parts of the same squeezed mass. In fact it is impossible that the folding should occur above without a corresponding crushing and thickening below.

Again, I am satisfied that Prof. Dana greatly under-estimates the amount of elevation by simple mashing as compared with folding: 1st, because folding is a superficial phenomenon and therefore always exposed to view, while crushing without folding is deep seated and only rarely exposed; 2d, because folding is *always* revealed by stratification, while crushing is only *sometimes* revealed by cleavage, for this structure is only developed in *suitable* materials; and 3d, because even after folding, extension upward may take place by mashing together of the folds, as I have shown in the early part of this paper.

I have spoken thus far of *closed* folds. In *open* folds such as occur on the skirts of mountain chains where the horizontal crushing has not been sufficient to bring the folds together, the case might seem to be different; but even in these there must be a mashing of the strata below each anticlinal and proportioned to its height, unless we assume a hollow arch beneath, or else such an arch supported by a liquid, an assumption which is expressly set aside in my paper.

Berkeley, Oct. 11, 1875.

ART. XXXVIII.—*Brief Contributions to Zoology from the Museum of Yale College.* No. XXXVII.—*Description of Mancasellus brachyurus, a new fresh water Isopod;* by O. HARGER.

THE genus *Asellopsis* * was proposed by the writer for the reception of *Asellus tenax* Smith, on account of the absence of mandibular palpi. A second species of this interesting genus has lately been collected by Mr. Fred. Mather, in Rockbridge

* This Journal, III, vol. vii, p. 601, 1874.

Co., Virginia. Since the name *Asellopsis* proves to have been preoccupied I propose in its place *Mancasellus*,* retaining *M. tenax* as the typical species, while the new species may be called *M. brachyurus*, from the short caudal stylets. This species resembles *M. tenax*, described and figured in the Report of the United States Commissioner of Fish and Fisheries, Part II, Report for 1872–3, p. 659, plate I, fig. 3, differing principally from it in the following points: The lateral margins of the head are entire; the proximal segment of the caudal stylets is short, being but little longer than the third segment of the antennæ; the rami are also short, the inner being much stronger and somewhat longer than the outer; in the males the propodus of the first pair of legs is armed with a prominent acute tooth on the palmar margin near the base, and, in the appendages of the seventh segment, the terminal portion of the outer pair is smaller and less expanded externally than in *M. tenax*, and the distal segment of the internal ramus of the inner pair is but little swollen at the base, and approaches the form seen in *Asellus communis* Say. The largest specimen measures 16^{mm}. in length exclusive of the antennæ and caudal stylets. The locality is worthy of mention as being on the Atlantic side of the Appalachian water-shed while *M. tenax* is yet known only from the Lakes.

Art. XXXIX.—*Professor Tyndall on Germs.*†

The author refers, in an introduction, to an inqury on the decomposition of vapors and the formation of actinic clouds by light, whereby he was led to experiment on the floating matter of the air. He refers to the experiments of Schwan, Schröder and Dusch, Schröder himself, to those of the illustrious French chemist Pasteur, to the reasoning of Lister and its experimental verification, regarding the filtering power of the lungs; from all of which he concluded, six years ago, that the power of developing life by the air, and its power of scattering light, would be found to go hand in hand. He thought the simple expedient of examining by means of a beam of light, while the eye was kept sensitive by darkness, the character of the medium in which their experiments were conducted, could not fail to be useful to workers in this field. But the method has not been much turned to account, and this year he thought it worth while to devote some time to the more complete demonstration of its utility.

* From *mancus*, maimed, and *Asellus*.

† On the Optical Deportment of the Atmosphere in reference to the Phenomena of Putrefaction and Infection. Abstract of a paper read before the Royal Society, January 13th, by Professor Tyndall, F.R.S. From Nature of Jan. 27 and Feb. 3.

He also wished to free his mind, and if possible the minds of others, from the uncertainty and confusion which now beset the doctrine of "spontaneous generation." Pasteur has pronounced it "a chimera," and expressed the undoubting conviction that, this being so, it is possible to remove parasitic diseases from the earth. To the medical profession, therefore, and through them to humanity at large, this question is one of the last importance. But the state of medical opinion regarding it is not satisfactory. In a recent number of the *British Medical Journal*, and in answer to the question, "in what way is contagium generated and communicated?" Messrs. Braidwood and Vacher reply that, notwithstanding "an almost incalculable amount of patient labor, the actual results obtained, especially as regards the manner of generation of contagium, have been most disappointing. Observers are even yet at variance whether these minute particles, whose discovery we have just noticed, and other disease germs, are always produced from like bodies previously existing, or whether they do not, under certain favorable conditions, spring into existence *de novo*."

With a view to the possible diminution of the uncertainty thus described, the author submits, without further preface to the Royal Society, and especially to those who study the etiology of disease, a description of the mode of procedure followed in this inquiry, and the results to which it has led.

A number of chambers, or cases, were constructed, each with a glass front, its top, bottom, back and sides being of wood. At the back is a little door which opens and closes on hinges, while into the sides are inserted two panes of glass, facing each other. The top is perforated in the middle by a hole two inches in diameter, closed air-tight by a sheet of india-rubber. This sheet is pierced in the middle by a pin, and through the pin-hole is passed the shank of a long pipette ending above in a small funnel. A circular tin collar two inches in diameter and one inch and a half high, surrounds the pipette, the space between both being packed with cotton-wool moistened by glycerine. Thus the pipette, in moving up and down, is not only firmly clasped by the india-rubber, but it also passes through a stuffing box of sticky cotton-wool. The width of the aperture closed by the india-rubber secures the free lateral play of the lower end of the pipette. Into two other smaller apertures in the top of the case are inserted, air-tight, the open ends of two narrow tubes, intended to connect the interior space with the atmosphere. The tubes are bent several times up and down, so as to intercept and retain the particles carried by such feeble currents as changes of temperature might cause to set in between the outer and the inner air.

The bottom of the box is pierced with two rows, sometimes with a single row of apertures, in which are fixed, air-tight, large test-tubes, intended to contain the liquid to be exposed to the action of the moteless air.

On Sept. 10 the first case of this kind was closed. The passage of a concentrated beam across it through its two side windows

then showed the air within it to be laden with floating matter. On the 13th it was again examined. Before the beam entered, and after it quitted the case, its track was vivid in the air, but within the case it vanished. Three days of quiet sufficed to cause all the floating matter to be deposited on the sides and bottom, where it was retained by a coating of glycerine, with which the interior surface of the case had been purposely varnished. The test-tubes were then filled through the pipette, boiled for five minutes in a bath of brine or oil, and abandoned to the action of the moteless air. During ebullition aqueous vapor rose from the liquid into the chamber, where it was for the most part condensed, the uncondensed portion escaping, at a low temperature through the bent tubes at the top. Before the brine was removed little stoppers of cotton-wool were inserted in the bent tubes, lest the entrance of the air into the cooling chamber should at first be forcible enough to carry motes along with it. As soon, however, as the ambient temperature was assumed by the air within the case, the cotton-wool stoppers were removed.

We have here the oxygen, nitrogen, carbonic acid, ammonia, aqueous vapor, and all the other gaseous matters which mingle more or less with the air of a great city. We have them, moreover, "untortured" by calcination and unchanged even by filtration or manipulation of any kind. The question now before us is, can air thus retaining all its gaseous mixtures, but self-cleansed from mechanically suspended matter, produce putrefaction? To this question both the animal and vegetable worlds return a decided negative.

Among vegetables, experiments have been made with hay, turnips, tea, coffee, hops, repeated in various ways with both acid and alkaline infusions. Among animal substances are to be mentioned many experiments with urine; while beef, mutton, hare, rabbit, kidney, liver, fowl, pheasant, grouse, haddock, sole, salmon, cod, turbot, mullet, herring, whiting, eel, oyster have been all subjected to experiment.

The result is that infusions of these substances exposed to the common air of the Royal Institution laboratory, maintained at a temperature of from 60° to 70° Fahr., all fell into putrefaction in the course of from two to four days. No matter where the infusions were placed, they were infallibly smitten. The number of the tubes containing the infusions was multiplied till it reached six hundred, but not one of them escaped infection.

On the other hand, in no single instance did the air, which had been proved moteless by the searching beam, show itself to possess the least power of producing Bacterial life or the associated phenomena of putrefaction. The power of developing such life in atmospheric air, and the power of scattering light, are thus proved to be indissolubly united.

The sole condition necessary to cause these long-dormant infusions to swarm with active life is the access of the floating matter of the air. After having remained for four months as pellucid as

distilled water, the opening of the back-door of the protecting case, and the consequent admission of the mote-laden air, sufficed in three days to render the infusions putrid and full of life.

That such life arises from mechanically suspended particles is thus reduced to ocular demonstration. Let us inquire a little more closely into the character of the particles which produce the life. Pour Eau-de-Cologne into water, a white precipitate renders the liquid milky. Or, imitating Brücke, dissolve clean gum mastic in alcohol, and drop it into water, the mastic is precipitated and milkiness produced. If the solution be very strong the mastic separates in curds; but by gradually diluting the alcoholic solution we finally reach a point where the milkiness disappears, the liquid assuming, by reflected light, a bright cerulean hue. It is, in point of fact, the color of the sky, and is due to a similar cause, namely, the scattering of light by particles, small in comparison to the size of the waves of light.

When this liquid is examined by the highest microscopic power it seems as uniform as distilled water. The mastic particles, though innumerable, entirely elude the microscope. At right angles to a luminous beam passing among the particles they discharge perfectly polarised light. The optical deportment of the floating matter of the air proves it to be composed, in part, of particles of this excessively minute character. When the track of a parallel beam in dusty air is looked at horizontally through a Nicol's prism, in a direction perpendicular to the beam, the longer diagonal of the prism being vertical, a considerable portion of the light from the finer matter is extinguished. The coarser motes, on the other hand, flash out with greater force, because of the increased darkness of the space around them. It is among the finest ultra-microscopic particles that the author shows the matter potential as regards the development of Bacterial life is to be sought.

But though they are beyond the reach of the microscope, the existence of these particles, foreign to the atmosphere but floating in it, is as certain as if they could be felt between the fingers, or seen by the naked eye. Supposing them to augment in magnitude until they come, not only within range of the microscope, but within range of the unaided senses. Let it be assumed that our knowledge of them under these circumstances remains as defective as it is now—that we do not know whether they are germs, particles of dead organic dust, or particles of mineral matter. Suppose a vessel (say a flower-pot) to be at hand filled with nutritious earth, with which we mix our unknown particles; and that in forty-eight hours subsequently buds and blades of well-defined cresses and grasses appear above the soil. Suppose the experiment when repeated over and over again to yield the same unvarying result. What would be our conclusion? Should we regard those living plants as the products of dead dust or mineral particles; or should we regard them as the offspring of living seeds? The reply is unavoidable. We should undoubtedly con-

sider the experiment with the flower-pot as clearing up our pre-existing ignorance; we should regard the fact of their producing cresses and grasses as proof positive that the particles sown in the earth of the pot were the seeds of the plants which have grown from them. It would be simply monstrous to conclude that they had been "spontaneously generated."

This reasoning applies word for word to the development of *Bacteria* from that floating matter which the electric beam reveals in the air, and in the absence of which no Bacterial life has been generated. There seems no flaw in this reasoning; and it is so simple as to render it unlikely that the notion of Bacterial life developed from dead dust can ever gain currency among the members of a great scientific profession.

A novel mode of experiment has been here pursued, and it may be urged that the conditions laid down by other investigators in this field, which have led to different results, have not been strictly attended to. To secure accuracy in relation to these alleged results, the latest words of a writer on this question, who has influenced medical thought both in this country and in America, are quoted. "We know," he says, "that boiled turnip or hay-infusions exposed to ordinary air, exposed to filtered air, to calcined air, or shut off altogether from contact with air, are more or less prone to swarm with *Bacteria* and vibriones in the course of from two to six days." Who the "we" are who possess this knowledge is not stated. The author is certainly not among the number, though he has sought anxiously for knowledge of the kind. He thus tests the statements in succession.

And first, with regard to the filtered air. A group of twelve large test-tubes were caused to pass air-tight through a slab of wood. The wood was coated with cement, in which, while hot, a heated "propagating glass" resembling a large bell jar was imbedded. The air within the jar was pumped out several times, air filtered through a plug of cotton-wool being permitted to supply its place. The test-tubes contained infusions of hay, turnip, beef, and mutton—three of each—twelve in all. They are as clear and cloudless at the present moment as they were upon the day of their introduction; while twelve similar tubes, prepared at the same time in precisely the same way and exposed to the ordinary air, are clogged with mycelium, mould, and *Bacteria*.

With regard to the calcined air, a similar propagating glass was caused to cover twelve other tubes filled with the same infusions. The "glass" was exhausted and carefully filled with air which had passed through a red-hot platinum tube, containing a roll of red-hot platinum gauze. Tested by the searching beam, the calcined air was found quite free from floating matter. Not a speck has invaded the limpidity of the infusions exposed to it, while twelve similar tubes placed outside have fallen into rottenness.

The experiments with calcined air took another form. Six years ago it was found that to render the laboratory air free from

floating matter, it was only necessary to permit a platinum wire heated to whiteness to act upon it for a sufficient time. Shades, containing pear juice, damson juice, hay and turnip-juice, and water of yeast, were freed from their floating matter in this way. The infusions were subsequently boiled and permitted to remain in contact with the calcined air. They are quite unchanged to the present hour, while the same infusions exposed to common air became mouldy and rotten along ago.

It has been affirmed that turnip and hay-infusions rendered slightly alkaline are particularly prone to exhibit the phenomena of spontaneous generation. This was not found to be the case in the present investigation. Many such infusions have been prepared, and they have continued for months without sensible alteration.

Finally, with regard to infusions wholly withdrawn from air, a group of test-tubes, containing different infusions, was boiled under a bell-jar filled with filtered air, and from which the air was subsequently removed as far as possible by a good air-pump. They are now as pellucid as they were at the time of their preparation, more than two months ago, while a group of corresponding tubes exposed to the laboratory air have all fallen into rottenness.

There is still another form of experiment on which great weight has been laid—that of hermetically sealed tubes. On April 6 last, a discussion on the "Germ Theory of Disease" was opened before the Pathological Society of London. The meeting was attended by many distinguished medical men, some of whom were profoundly influenced by the arguments, and none of whom disputed the facts brought forward against the theory on that occasion. The following important summary of these was then given:—"With the view of settling these questions, therefore, we may carefully prepare an infusion from some animal tissue, be it muscle, kidney, or liver; we may place it in a flask whose neck is drawn out and narrowed in the blowpipe-flame, we may boil the fluid, seal the vessel during ebullition, and keeping it in a warm place, may await the result, as I have often done. After a variable time the previously heated fluid within the hermetically sealed flask swarms more or less plentifully with *Bacteria* and allied organisms."

Previous to reading this statement the author had operated upon tubes of hay and turnip-infusions, and upon twenty-one tubes of beef, mackerel, eel, oyster, oat-meal, malt, and potato, hermetically sealed while boiling, not by the blowpipe, but by the far more handy spirit-lamp flame. In no case was any appearance whatever of *Bacteria* or allied organisms observed. The perusal of the discussion just referred to caused the author to turn again to muscle, liver, and kidney, with a view of varying and multiplying the evidence. Fowl, pheasant, snipe, partridge, plover, wild duck, beef, mutton, heart, tongue, lungs, brains, sweetbread, tripe, the crystalline lens and vitreous humor of an ox, herring, haddock, mullet, codfish, sole, were all embraced in the experiments. There was neither mistake nor ambiguity about the result. One hun-

dred and thirty-nine of the flasks operated on were exhibited, and not one of this cloud of witnesses offers the least countenance to the assertion that liquids within flasks, boiled and hermetically sealed, swarm, subsequently, more or less plentifully with *Bacteria* and allied organisms.

The evidence furnished by this mass of experiments, that errors either of preparation or observation have been committed, is, it is submitted, very strong. But to err is human; and in an inquiry so difficult and fraught with such momentous issues, it is not error, but the persistence in error by any of us, for dialectic ends, that is to be deprecated. The auther shows by illustrations the risks of error run by himself. On Oct. 21 he opened the back-door of a case containing six test-tubes filled with an infusion of turnip which had remained perfectly clear for three weeks, while three days sufficed to crowd six similar tubes exposed to mote-laden air with *Bacteria*. With a small pipette he took specimens from the pellucid tubes, and placed them under the microscope. One of them yielded a field of Bacterial life, monstrous in its copiousness. For a long time he tried vainly to detect any source of error, and was perfectly prepared to abandon the unvarying inference from all the other experiments, and accept the result as a clear exception to what had previously appeared to be a general law. The cause of his perplexity was finally traced to the tiniest speck of an infusion containing *Bacteria*, which had clung by capillary attraction to the point of one of his pipettes.

Again, three tubes containing infusions of turnip, hay, and mutton, were boiled on Nov. 2 under a bell-jar containing air so carefully filtered that the most searching examination by a concentrated beam failed to reveal a particle of floating matter. At the present time every one of the tubes is thick with mycelium and covered with mould. Here surely we have a case of spontaneous generation. Let us look to its history.

After the air has been expelled from a boiling liquid it is difficult to continue the ebullition without "bumping." The liquid remains still for intervals, and then rises with sudden energy. It did so in the case now under consideration, and one of the tubes boiled over, the liquid over-spreading the resinous surface in which the bell-jar was imbedded, and on which, doubtless, germs had fallen. For three weeks the infusions had remained perfectly clear. At the end of this time, with a view of renewing the air of the jar, it was exhausted, and refilled by fresh air which had passed through a plug of cotton-wool. As the air entered, attention was attracted by two small spots of penicillium resting on the liquid which had boiled over. It was at once remarked that the experiment was a dangerous one, as the entering air would probably detach some of the spores of the penicillium and diffuse them in the bell-jar. This was, therefore, filled very slowly, so as to render the disturbance a minimum. Next day, however, a tuft of mycelium was observed at the bottom of one of the three tubes, namely that containing the hay-infusion. It has

by this time grown so as to fill a large portion of the tube. For nearly a month longer the two tubes containing the turnip and mutton-infusions maintained their transparency unimpaired. Late in December the mutton-infusion, which was in dangerous proximity to the outer mould, showed a tuft upon its surface. The beef-infusion continued bright and clear for nearly a fortnight longer. The recent cold weather caused me to add a third gas-stove to the two which had previously warmed the room in which the experiments are conducted. The warmth of this stove played upon one side of the bell-jar: and on the day after the lighting of the stove, the beef-infusion gave birth to a tuft of mycelium. In this case the small spots of penicillium might have readily escaped attention; and had they done so we should have had three cases of "spontaneous generation" far more striking than many that have been adduced.

In further illustration of the dangers incurred in this field of inquiry the author refers to the excellent paper of Dr. Roberts on Biogenesis, in the "Philosophical Transactions" for 1874. Dr. Roberts fills the bulb of an ordinary pipette to about two-thirds of its capacity with the infusion to be examined. In the neck of the pipette he places a plug of dry cotton-wool. He then hermetically seals the neck and dips the bulb into boiling water or hot oil, where he permits it to remain for the requisite time. Here we have no disturbance from ebullition, and no loss by evaporation. The bulb is removed from the hot water and permitted to cool. The sealed end of the neck is then filed off, the cotton-wool alone interposing between the infusion and the atmosphere.

The arrangement is beautiful, but it has one weak point. Cotton-wool free from germs is not to be found, and the plug employed by Dr. Roberts infallibly contained them. In the gentle movement of the air to and fro as the temperature changed, or by any shock, jar, or motion to which the pipette might be subjected, we have certainly a cause sufficient to detach a germ now and then from the cotton-wool which would fall into the infusion and produce its effect. Probably, also, condensation occurred at times in the neck of the pipette; the water of condensation carrying back from the cotton-wool the seeds of life. The fact of fertilization being so rare as Dr. Roberts found it to be is a proof of the care with which his experiments were conducted. But he did find cases of fertillization after prolonged exposure to the boiling temperature; and this caused him to come to the conclusion that under certain rare conditions spontaneous generation may occur. He also found that an alkalised hay-infusion was so difficult to sterilise that it was capable of withstanding the boiling temperasure for hours without losing its power of generating life. The most careful experiments have been made with this infusion. Dr. Roberts is certainly correct in assigning to it superior nutritive power. But in the present inquiry five minutes boiling sufficed to completely sterilise the infusion.

Summing up this portion of his inquiry, the author remarks that he will hardly be charged with any desire to limit the power and potency of matter. But holding the notions he does upon this point, it is all the more incumbent on him to affirm that as far as inquiry has hitherto penetrated, life has never been proved to appear independently of antecedent life.

Though the author had no reason to doubt the general diffusion of germs in the atmosphere, he thought it desirable to place the point beyond question. At Down, Mr. Darwin, Mr. Francis Darwin; at High Elms, Sir John Lubbock; at Sherwood, near Tunbridge Wells, Mr. Siemens; at Pembroke Lodge, Richmond Park, Mr. Rollo Russell; at Heathfield Park, Messrs. Hamilton; at Greenwich Hospital, Mr. Hirst; at Kew, Dr. Hooker; and at the Crystal Palace, Mr. Price, kindly took charge of infusions, every one of which became charged with organisms. To obtain more definite insight regarding the diffusion of atmospheric germs, a square wooden tray was pierced with 100 holes, into each of which was dropped a short test-tube. On Oct. 23, thirty of these tubes were filled with an infusion of hay, thirty-five with an infusion of turnip, and thirty-five with an infusion of beef. The tubes, with their infusions, had been previously boiled, ten at a time, in an oil-bath. One hundred circles were marked on paper so as to form a map of the tray, and every day the state of each tube was registered upon the corresponding circle. In the following description the term "cloudy" is used to denote the first stage of turbidity; distinct but not strong. The term "muddy" is used to denote thick turbidity.

One tube of the 100 was first singled out and rendered muddy. It belonged to the beef group, and it was a whole day in advance of all the other tubes. The progress of putrefaction was first registered on Oct. 26; the "map" then taken may be thus described:

Hay.—Of the thirty specimens exposed one had become "muddy"—the seventh in the middle row reckoning from the side of the tray nearest the stove. Six tubes remained perfectly clear between this muddy one and the stove, proving that differences of warmth may be overridden by other causes. Every one of the other tubes containing the hay infusion showed spots of mould upon the clear liquid.

Turnip.—Four of the thirty-five tubes were very muddy, two of them being in the row next the stove, one four rows distant, and the remaining one seven rows away. Besides these six tubes had become clouded. There was no mould on any of the tubes.

Beef.—One tube of the thirty-five was quite muddy, in the seventh row from the stove. There were three cloudy tubes, while seven of them bore spots of mould.

As a general rule organic infusions exposed to the air during the autumn remained for two days or more perfectly clear. Doubtless from the first germs fell into them, but they required time to be hatched. This period of clearness may be called the "period of latency," and indeed it exactly corresponds with what

is understood by this term in medicine. Toward the end of the period of latency, the fall into a state of disease is comparatively sudden; the infusion passing from perfect clearness to cloudiness more or less dense in a few hours.

Thus the tube placed in Mr. Darwin's possession was clear at 8.30 A. M. on Oct. 19, and cloudy at 4.30 P. M. Seven hours, moreover, after the first record of our tray of tubes, a marked change had occurred. It may be thus described:—Instead of one, eight of the tubes containing hay-infusion had fallen into uniform muddiness. Twenty of these had produced Bacterial slime, which had fallen to the bottom, every tube containing the slime being covered by mould. Three tubes only remained clear, but with mould upon their surfaces. The muddy turnip-tubes had increased from four to ten; seven tubes were clouded, while eighteen of them remained clear, with here and there a speck of mould on the surface. Of the beef, six were cloudy and one thickly muddy, while spots of mould had formed on the majority of the remaining tubes. Fifteen hours subsequent to this observation, viz. on the morning of Oct. 27, all the tubes containing hay-infusion were smitten, though in different degrees, some of them being much more turbid than others. Of the turnip-tubes, three only remained unsmitten, and two of these had mould upon their surfaces. Only one of the thirty-five beef-infusions remained intact. A change of occupancy, moreover, had occurred in the tube which first gave way. Its muddiness remained gray for a day and a half, then it changed to bright yellow green, and it maintained this color to the end. On the 27th every tube of the hundred was smitten, the majority with uniform turbidity; some, however, with mould above and slime below, the intermediate liquid being tolerably clear. The whole process bore striking resemblance to the propagation of a plague among a population, the attacks being successive and of different degrees of virulence.

From the irregular manner in which the tubes are attacked, we may infer that, as regards *quantity*, the distribution of the germs in the air is not uniform. The singling out, moreover, of one tube of the hundred by the particular *Bacteria* that develop a green pigment, shows that, as regards *quality*, the distribution is not uniform. The same absence of uniformity was manifested in the struggle for existence between the *Bacteria* and the penicillium. In some tubes the former were triumphant; in other tubes of the same infusion the latter was triumphant. It would seem also as if a want of uniformity as regards *vital vigor* prevailed. With the self-same infusion the motions of the *Bacteria* in some tubes were exceedingly languid, while in other tubes the motions resembled a rain of projectiles, being so rapid and violent as to be followed with difficulty by the eye. Reflecting on the whole of this, the author concludes that the germs float through the atmosphere in groups or clouds, with spaces more sparsely filled between them. The touching of a nutritive fluid by a Bacterial cloud would naturally have a different effect from the touching of

it by the interspace between two clouds. But as in the case of a mottled sky, the various portions of the landscape are successively visited by shade, so, in the long run, are the various tubes of our tray touched by the Bacterial clouds, the final fertilization or infection of them all being the consequence. The author connects these results with the experiments of Pasteur on the non-continuity of the cause of so-called spontaneous generation, and with other experiments of his own.*

On the 9th of November a second tray containing one hundred tubes filled with an infusion of mutton was exposed to the air. On the morning of the 11th six of the ten nearest the stove had given way to putrefaction. Three of the rows most distant from the stove had yielded, while here and there over the tray particular tubes were singled out and smitten by the infection. Of the whole tray of one hundred tubes, twenty-seven were either muddy or cloudy on the 11th. Thus, doubtless, in a contagious atmosphere, are individuals successively struck down. On the 12th all the tubes had given way, but the differences in their contents were extraordinary. All of them contained *Bacteria*, some few, others in swarms. In some tubes they were slow and sickly in their motions, in some apparently dead, while in others they darted about with rampant vigor. These differences are to be referred to changes in the germinal matter, for the same infusion was presented everywhere to the air. Here also we have a picture of what occurs during an epidemic, the difference in number and energy of the Bacterial swarms resembling the varying intensity of the disease. It becomes obvious from these experiments that of two individuals of the same population, exposed to a contagious atmosphere, the one may be severely, the other lightly attacked, though the two individuals may be as identical as regards susceptibility as two samples of one and the same mutton infusion.

The author traces still further the parallelism of these actions with the progress of infectious disease. The *Times* of January 17th contained a letter on Typhoid Fever signed "M.D.," in which occurs the following remarkable statement:—"In one part of it (Edinburgh), congregated together and inhabited by the lowest of the population, there are, according to the Corporation return for 1874, no less than 14,319 houses or dwellings—many under one roof, on the 'flat' system—in which there are no house connections whatever with the street sewers, and, consequently, no water-closets. To this day, therefore, all the excrementitious and other refuse of the inhabitants is collected

* In hospital practice the opening of a wound during the passage of a Bacterial cloud would have an effect very different from the opening of it in the interspace between two clouds. Certain caprices in the behavior of dressed wounds may possibly be accounted for in this way. Under the heading "Nothing new under the Sun," Prof. Huxley has just sent me the following remarkable extract:—"Uebrigens kann man sich die in der Atmosphäre schwimmenden Thierchen wie Wolken denken, mit denen ganz leere Luftmassen, ja ganze Tage völlig reinen Luftverhältnisse wechseln." (Ehrenberg, "Infusions Thierchen," 1838, p. 525.) The coincidence of phraseology is surprising, for I knew nothing of Ehrenberg's conception. My "clouds," however, are but small miniatures of his.

in pails or pans, and remains in their midst, generally in a partitioned-off corner of the living room, until the next day, when it is taken down to the streets and emptied into the Corporation carts. Drunken and vicious though the population be, herded together like sheep, and with the filth collected and kept for twenty-four hours in their very midst, it is a remarkable fact that typhoid fever and diphtheria are simply unknown in these wretched hovels."

This case has its analogue in the following experiment, which is representative of a class. On Nov. 30 a quantity of animal refuse, embracing beef, fish, rabbit, hare, was placed in two large test-tubes opening into a protecting chamber containing six tubes. On Dec. 13, when the refuse was in a state of noisome putrefaction, infusions of whiting, turnip, beef and mutton were placed in the other four tubes. They were boiled and abandoned to the action of the foul "sewer gas" emitted by their two putrid companions. On Christmas-day the four infusions were limpid. The end of the pipette was then dipped into one of the putrid tubes, and a quantity of matter comparable in smallness to the pock-lymph held on the point of a lancet was transferred to the turnip. Its clearness was not sensibly affected at the time; but on the 26th it was turbid throughout. On the 27th a speck from the infected turnip was transferred to the whiting; on the 28th disease had taken entire possession of the whiting. To the present hour the beef and mutton tubes remain as limpid as distilled water. Just as in the case of the living men and women in Edinburgh, no amount of fetid gas had the power of propagating the plague, so long as the organisms which constitute the true contagium did not gain access to the infusions.

The universal prevalence of the germinal matter of *Bacteria* in water has been demonstrated with the utmost evidence by the experiments by Dr. Burdon Sanderson. But the germs in water are in a very different condition, as regards readiness for development, from those in air. In water they are thoroughly wetted, and ready, under the proper conditions, to pass rapidly into the finished organisms. In air they are more or less desiccated, and require a period of preparation more or less long to bring them up to the starting-point of the water-germs. The rapidity of development in an infusion infected by either a speck of liquid containing *Bacteria* or a drop of water is extraordinary. On Jan. 4 a thread of glass almost as fine as a hair was dipped into a cloudy turnip infusion, and the tip only of the glass fiber was introduced into a large test-tube containing an infusion of red mullet. Twelve hours subsequently the perfectly pellucid liquid was cloudy throughout. A second test-tube containing the same infusion was infected with a single drop of the distilled water furnished by Messrs. Hopkin and Williams; twelve hours also sufficed to cloud the infusion thus treated. Precisely the same experiments were made with herring with the same result. At this season of the year several days' exposure to the air are

to produce so great an effect. On Dec. 31 a strong turnip-infusion was prepared by digesting thin slices in distilled water at a temperature of 120° F. The infusion was divided between four large test-tubes, in one of which it was left unboiled, in another boiled for five minutes, in the two remaining ones boiled, and after cooling infected with one drop of beef-infusion containing *Bacteria*. In twenty-four hours the unboiled tube and the two infected ones were cloudy, the unboiled tube being the most turbid of the three. The infusion here was peculiarly limpid after digestion; for turnip it was quite exceptional, and no amount of searching with the microscope could reveal in it at first the trace of a living Bacterium; still germs were there which, suitably nourished, passed in a single day into Bacterial swarms without number. Five days have not sufficed to produce an effect approximately equal to this in the boiled tube, which was uninfected but exposed to the common laboratory air.

There cannot, moreover, be a doubt that the germs in the air differ widely among themselves as regards *preparedness* for development. Some are fresh, others old; some are dry, others moist. Infected by such germs the same infusion would require different lengths of time to develop Bacterial life. This remark applies to and explains the different degrees of rapidity with which epidemic disease acts upon different people. In some the hatching-period, if it may be called such, is long, in some short, the differences depending upon the different degrees of preparedness of the contagium.

The author refers with particular satisfaction to the untiring patience, the admirable mechanical skill, the veracity in thought, word, and deed, displayed throughout this first section of a large and complicated inquiry by his assistant, Mr. John Cottrell, who was zealously aided by his junior colleague, Mr. Frank Valter.

NOTE. *Jan.* 31.—The notion that the author limited himself to temperatures of 60° and 70° Fahr. is an entire misconception. But more of this anon.

ART. XL.—*Discovery of a new Planet*; by C. H. F. PETERS. (From a letter to one of the Editors, dated Litchfield Observatory of Hamilton College, Clinton, N. Y., Feb. 26, 1876.)

A new planet, eleventh magnitude, was first seen here on the night of the 20th inst., and, after several days of bad weather, again observed on the following nights. The positions are as follows:

1876.	Ham. Col. m. t.	α(160)	δ(160)	
Feb. 20,	15^h —^{m} —^{s}	$10^h\ 20^m\ 59^s$	$+14°\ 14.'1$	
" 24,	12 25 41	10 17 26·8	+14 28 45	(12 comp.)
" 25,	11 46 11	10 16 33·92	+14 32 26·0	(15 comp.)

On the first night only an approximate observation was made. For the second night the comparison star still needs determination by means of the meridian circle.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On a Crystallized Hydrate of Hydrochloric acid.*—Pierre and Puchot have observed that, when a saturated solution of hydrochloric acid gas is cooled to —21° or —22° C., the dry gas being passed continuously into the liquid, after a few minutes the temperature rises to —18° and an abundant crystallization begins, during which the temperature remains constant at —18°. Before the crystallization commences, there is always observed this lowering of 3° or 4° in temperature, which is a phenomenon analogous to sursaturation. A synthetic experiment showed that, to produce the crystals, the water absorbed about its own weight of the gas; and hence showed that the probable formula was HCl. $(H_2O)_2$. In the air the crystals decompose readily, giving off dense fumes of hydrogen chloride. In a flask, kept near 0°, they slowly melt, the temperature remaining at —18°; in one experiment 115 grams of the crystals required an hour and a quarter to melt. Water dissolves them readily. Since they sink in the solution where they are formed, they must be denser than it. They set free the gas in melting, and hence must contain more of it than the mother liquors. In the analysis, a known weight of the drained crystals was treated with a definite quantity of distilled water, in amount sufficient to prevent the evolution of gas. The chlorine was then determined in the solution, and from this the ratio between the HCl and the H_2O could be calculated. In the first two determinations, the ratio was 1 : 2·19; in the second it was 1 : 2·085 and 1 : 2·075. Hence the authors conclude upon the formula $HCl.(H_2O)_2$; this is the best defined hydrate of hydrochloric acid yet observed. A mixture of snow two parts and hydrochloric acid one part gives a temperature of —32°C.; or of —35° if the materials are previously cooled.—*C. R.*, lxxxii, 45, Jan. 1876. G. F. B.

2. *On the Decomposition of Water by Platinum.*—Sainte Claire Deville and Debray state that if potassium cyanide be heated in a glass tube to 500° or 600°, in the vicinity of a boat full of warm water, the tube having been previously exhausted, the pressure rises to half an atmosphere, and remains constant for hours; but if, before the operation, some platinum sponge has been mixed with the cyanide, hydrogen is abundantly evolved, and a potassio-platinum cyanide is formed. This hydrogen contains from 4½ to 12 per cent of carbonous oxide, produced according to the following reaction.

$$(KCN)_2+(H_2O)_4=K_2CO_3+(NH_3)_2+H_2+CO$$

If the principal reaction in the foregoing experiment be written—

$$(KCy)_4+(H_2O)_2=PtCy_2(KCy)_2+(KOH)_2+H_2$$

it would appear as if the platinum decomposed water under the influence of the potassic cyanide. But the authors show from thermal considerations that the potassium hydrate formed is really the important product; that in its formation the greatest amount of

heat is developed, and hence it is really the determinant of the reaction. From the same causes, a boiling concentrated solution of potassium cyanide attacks platinum, setting free hydrogen; an experiment the authors recommend as a convenient one to illustrate the principles of thermo-chemistry upon the lecture-table. So also a boiling solution of mercuric cyanide does not attack platinum unless potassium cyanide be present; then the mercury at once separates.—*C. R.*, lxxxii, 241, Jan. 1876. G. F. B.

3. *On a New Compound of Sulphur and Oxygen.*—For many years it has been known that the action of sulphur on sulphuric oxide or on disulphuric acid produces an intense blue color. R. WEBER has successfully investigated the cause of this color, and has shown that it is due to a new oxide of sulphur which he has isolated. To prepare it, a portion of sulphuric oxide is prepared, containing some sulphuric acid, and into this is thrown, in small portions, carefully dried flowers of sulphur. At the instant of contact the sulphur is converted into dark blue liquid drops which sink to the bottom of the liquid and there solidify. Care should be taken to keep the temperature at 15° C., since below this point the whole liquid solidifies, and above it the blue body decomposes. After the operation, the excess of liquid is poured off, the blue crystalline crusts are drained and the excess of sulphuric oxide driven off at a temperature not exceeding blood heat. Bluish green crusts are thus obtained, which are very friable and which have a structure similar to malachite. They decompose without fusion slowly at ordinary temperatures, more rapidly on heating, evolving sulphurous oxide and leaving sulphur behind. In a cool place the decomposition is so slow that the substance may readily be weighed for analysis. Moist air decomposes it rapidly and it hisses when thrown into water. Alcohol and ether also decompose it, and set free sulphur. A mean of five closely accordant analyses showed that it contained 57·12 per cent of sulphur; thus giving it the formula S_2O_3. The author names it sulphur sesquioxide or dithionic oxide. No compounds of it have yet been made. Selenium gives an analogous compound having the formula $SeSO_3$. It is dirty-green in mass, yellow in powder.—*Pogg. Ann.*, clvi, 531 Dec., 1875.

G. F. B.

4. *On the Purification of Carbon disulphide.*—FRIEDBERG proposes to effect the final purification of carbon disulphide by treating it with fuming nitric acid. The crude disulphide is first purified by repeated distillation with a vegetable fat, such as palm oil, and is then treated with the acid and frequently agitated. After twenty-four hours two layers are observed, nearly of the same color, the red vapors of the acid having been absorbed by the CS_2. If water be added, the disulphide becomes reddish violet, and this separated from the acid, washed, and gently heated, gives up pure disulphide in the distillate, while the violet-colored solution of the disulphide remains behind, and is not broken up except at a higher temperature. The colorless distillate, washed with water dried and distilled, is chemically pure. The author is investigating the solvent

power of this substance for gaseous substances.—*Ber. Berl. Chem. Ges.*, viii, 1616, Jan. 1876. G. F. B.

5. *The New Metal Gallium.*—In the session of the French Academy on September 20, the Secretary opened a sealed note deposited by LECOQ DE BOISBAUDRAN, the first paragraph of which reads thus:—"Day before yesterday, on Friday the 27th of August, 1875, between three and four o'clock in the afternoon, I obtained indications of the probable existence of a new simple body among the products of the chemical examination of a blende coming from the mine of Pierrefitte, valley of Argeles, Pyrenees." The evidence relied on to prove this discovery, a part of which evidence was given in the sealed note and another part in a note read at the same meeting, is: (1) the oxide (or perhaps a basic salt) is precipitated slowly by metallic zinc in a solution containing chlorides and sulphates; (2) its salts are easily precipitated by barium carbonate in the cold; and (3) it gives a spectrum showing two violet lines of wave lengths 417 and 404 respectively. In all its other chemical reactions, it closely resembles zinc; though in the precipitations it has always the preference when these are incomplete. To the metal thus indicated, Lecoq de Boisbaudran gave the name *Gallium.* In a more recent paper he gives additional facts regarding the new metal, which he has been able to free almost entirely from zinc. From it he has prepared a salt which he believes to be gallium-alum. It is soluble in cold water, but is decomposed on heating, unless acetic acid be present. It crystallizes in octahedrons and cubes, presenting the appearance of common alum, especially under the microscope; the crystals do not polarize light. Placed in a super-saturated solution of ammonio-aluminum alum, they act as nuclei and begin to grow. Treated with ammonia, a part only of the oxide is thrown down. In ammoniacal solution, the metal is precipitated by electrolysis on the negative electrode. In the first trial 1·6 milligrams were deposited in 4½ hours; in the second, 3·4 milligrams was deposited in 5 hours 40 minutes. (This sample was submitted to the Academy.) The metal adhered strongly to the platinum on which it was deposited. When burnished its surface is brilliant, and has a color between silver and platinum. With a feeble current, the metal comes down frosted and crystalline. It does not decompose water at ordinary temperatures, and tarnishes slowly in the open air. With HCl, it evolves hydrogen. On the evidence of the alum, he fixes the formula of the oxide as Ga_2O_3, and assigns the metal to the aluminum group. In a subsequent note, the author gives the results of the more accurate measurement of the wave-lengths of the two lines of the gallium spectrum, α and β. A concentrated solution of the chloride gave only the two lines at first observed, of wave-lengths 417 and 403·1, the former being the stronger.—*C. R.*, lxxxi, 493 (Sept.) 1100, (Dec. 1875); lxxxii, 168, Jan. 1876. G. F. B.

6. *Conductibility of Gases.*—M. A. WINKELMANN has measured the conductibility of gases for heat by an apparatus like that of Stephan, except that a peculiar manometer and method of

closure is used. The apparatus consists of a brass cylinder serving as an air thermometer enclosed in a second concentric cylinder. The top of the inner cylinder may be unscrewed and it carries a conical cavity in which is placed a rubber cork with a hole through it. A glass tube passes through this hole and a metallic cap screwed on below the rubber presses it against the glass. A similar closure carries the tube through the outer cylinder. The latter is connected with a mercury pump. A comparison of the time and variations of the pressure when the outer cylinder was immersed in ice-water gave the following coefficients of conductibility:

Name.	Conduct.
Air	·0000525
Hydrogen	·0003324
Carbonic Acid	·0000317
Ethyl	·0000414
Marsh Gas	·0000647
Nitric Oxide	·0000460
Carbonic Oxide	·0000510
Oxygen	·0000563
Nitrous Oxide	·0000363
Nitrogen	·0000524

—*Pogg. Ann.* clvi, 497. E. C. P.

7. *Thermal Properties of Liquids.*—M. PICTET has applied the mechanical theory of heat to the study of volatile liquids, making use of the experiments of Regnault, and deduces the following simple relations between their latent heats, atomic weight and vapor tension:

(1.) The cohesion of all liquids is constant.

(2.) The differential coefficient of the Naperian logarithm of the tension divided by the temperature is constant for all liquids when referred to the same pressure and temperature.

(3.) The latent heat of all liquids referred to the same pressure, multiplied by the atomic weight referred to the same temperature, gives a constant product.

(4.) For all liquids the difference of the internal latent heats at any two temperatures, multiplied by the atomic weight is a constant number.

It thus appears that quantities at first sight wholly independent are really connected by very simple relations, which dispense with long empirical formulas based on observations more or less open to criticism.

Furthermore, admitting the law of Dulong and Petit for specific heats, we can further say that the latent heat of all liquids are multiples of their specific heats.—*Bibl. Univ.*, ccxvii, 66.

E. C. P.

8. *Dependence of Electrical Resistance on the Motion of the Conductor.*—M. EDLUND has brought to bear a new argument in favor of his theory of electricity, by showing that the resistance of a conductor is affected by its motion. Water is allowed to

flow through a long tube having three electrodes of gold wire admitted at its ends and center. A battery of two Daniell's cells has one terminal connected with the center electrode, and the other with two of the terminals of a delicate differential astatic galvanometer. The two end electrodes are connected with the other terminals of the galvanometer. The current from the battery divides, and half passes through the tube in each direction. By suitably varying the resistance, the galvanometer needle will now be at rest. When the water is caused to flow through the tube, however, the resistance in one direction will be increased, and that in the other diminished, since, according to Edlund's theory the current is proportional to the amount of ether flowing through a given section per second. Accordingly the needle should deviate, as, in fact, it does. To eliminate the effects of polarization, the current was inverted without changing the result. That the deviations may be regular, it is essential that the liquid should have a great resistance and the amount of deviation is almost independent of this resistance. Two series of observations were made, one with distilled water, the other with alcohol and water, and gave similar results. A third series with aqueduct water gave the same result. Finally, equal currents were sent in opposite directions through the pipe, when they produced no effect on the needle, but as soon as the liquid was set in motion a deviation was always obtained indicating that the resistance was greater in one direction than in the other. These two methods of observation lead to the same result, foreseen by the theory of Edlund, namely, that the galvanic resistance diminishes if the conductor moves in the same direction as the galvanic current and increases on the contrary if the other two currents move in opposite directions.—*Royal Swedish Acad.*, III., *No.* 11, *Phil. Mag.*, l, 89. E. C. P.

9. *Electric Spark with large Batteries.*—Messrs. Warren de la Rue and H. W. Muller presented to the Royal Society at a recent meeting a paper having the following title: On the length of the Spark from a Battery of 600, 1200, 1800 and 2400 rod-chloride of Silver, and some Phenomena attending the discharge of 5640 cells. A year ago some experiments on the stratification of the discharge in vacuo of a battery of 1080 cells were described. This battery has now been augmented to 5640 cells, and two other batteries will soon be added making 9120 cells. Having completed 2400 cells and charged them up in a single day, they were exactly in the same condition as to electro-motive force and internal resistance, consequently they afforded the means of testing the truth of the law of the length of the spark in a manner more efficacious than had hitherto been obtained, the more especially as by the use of paraffin corks and other precautions we had obtained an excellent insulation. A discharger with a micrometer-screw was constructed by which the length of spark could be measured to ·001 of an inch, or by estimation to one-tenth of that quantity. In making measurements the terminals were separated

to a greater distance than the anticipated striking-distance and gradually approached until the spark passed. The discharger was then detached from the battery, and after reading the scale, connected with a separate battery of 10 cells with a detector-galvanometer in circuit. The terminals were again approached until the motion of the galvanometer indicated contact between them. The scale was again read, and the change in reading gave the required length of spark. With 600, 1200, 1800 and 2400 cells the striking-distances were found to be ·0033, ·0130, ·0345 and ·0535 inches. These numbers are nearly proportional to the square of the number of cells, which would give the distances ·0033, 0132, ·0297, ·0528. The length of spark is much influenced by the form of the terminal. Generally a copper-pointed plane was employed and the current reversed 352 times per second by a revolving commutator, or a double key discharger.

When the point was negative, a glow in form like a paraboloid was seen surrounding it long before the spark passed, and as the distance was diminished gradually extending to the positive terminal. With 1800 cells the glow was seen at a distance of ·0545 and with 2400 cells at a distance of ·0865 inches. Moreover when the disc was positive it became covered with a peach-like bloom which became stronger in the center as the terminals were approached, giving rise to Newton's rings. This effect was next studied with the whole series of 5640 cells. The glow was now visible at 1·073 in. and the spark passed at ·139. Replacing the flat disc by one that was slightly convex the glow occurred at 1·124 in. and the spark at ·140. Reversing, the current gave sparks of ·154 and ·164 in.

To ascertain whether a current really passed when the glow appeared, vacuum-tubes were interposed when they were illuminated even before the glow appeared. Of course the striking distance was in this case shortened. With a hydrogen tube having a resistance of 190,000 ohms the glow occurred at ·939 and the spark at ·092 inches. A 31-inch tube was brilliantly illuminated when interposed between one terminal and the battery, when the terminals were separated to the extreme range of the discharger or 1·2 in. and before any glow was visible at the negative electrode. Later a current was obtained with the negative point 5·1 in. distant from a positive plate 6 in. in diameter.

Considerable difficulty was experienced in measuring the resistance of the tubes, and it soon appeared that this resistance rapidly increased as the current passed. After a time, however, they recovered their original resistance. Ultimately it was found to be better to discard the indications of the galvanometer and to rely solely on the appearance of a luminosity in the tubes placed on one side of Wheatstone's bridge as soon as the insertion of a balancing resistance was made in the other. A curious conclusion is derived from the law that the length of spark is proportional to the square of the number of cells, if it proves to be correct. One cell would give a spark about ·00000001 in. long while a hundred

thousand which come within the limit of experimental possibility would give a spark about 92 inches long. Probably a million would never be made but they should give a spark 9166 inches or 764 feet long.—*Nature*, xiii, 277. E. C. P.

10. *Acoustics.* Letter to the editors by Professor A. M. MAYER, dated Stevens Institute of Technology, Hoboken, New Jersey, March 17th, 1876.—Gentlemen: Having in hand researches whose completion will occupy several months, I desire to place on record my invention of the two following methods of research. The first is a plan for *the determination of the relative intensities of sounds of the same pitch.* The second is *a method of determining the direction of sounds.* I request the privilege of being permitted first to attempt to develop these ideas after I have finished the original work which at present occupies all my leisure.

First method. A loose membrane, or a slip of gold or aluminium foil, is placed anywhere between the centers of origin of two sounds of the same pitch. The plane of this membrane is at right angles to the line connecting these sonorous centers. If both sides of the membrane are simultaneously acted on by sonorous vibrations of the same phase and of equal intensity, the membrane will remain at rest. The above condition is thus attained. Attach to the center of the membrane a short delicate glass thread whose end can be observed through a microscope, or, place a reflecting metallic film on the central part of the membrane so that one can observe the motion of a beam of light reflected therefrom. If we place, at hazard, the membrane between the sonorous centers it is probable that it will be set in vibration. Now if it is moved from its position its vibrations will either increase or decrease in amplitude. Move it in the direction that causes the amplitude of the vibrations to decrease, and until the vibrations have a minimum of swing. The membrane is now in a plane where the phases of vibration are the same but of *unequal* intensity. The membrane is now moved one-half wave-length either from or toward one of the sonorous centers and is thus brought into another plane of minimum vibration. Thus move the membrane until it is brought into that plane where vibrations of the membrane are either entirely destroyed or have their least amplitude. If the membrane vibrates, then move it and the source of one of the sounds so that they both approach to or recede from the other sonorous center always by the same quantity. This is accomplished by moving a board to which is attached the membrane and one of the sources of sound. By the last adjustment we can soon reach a plane where the membrane remains at rest and where the intensities of the two sonorous vibrations are equal. It appears that I have thus devised a *phonometer* which is analogous to the photometer of Bunsen.

The above method appears to me preferable to the phonometer I described in this Journal, Feb., 1873. There are objections to the use of resonators and reflection which I cannot here explain.

Second method. If the plane of a free membrane be placed at

right angles to a wave-front it cannot vibrate, for both of its sides are simultaneously acted on by impulses of the same phase and of equal energy. Thus, by bringing the plane of a membrane into that azimuth where it remains at rest we shall have found the plane passing through the center of origin of the sound. I also propose the use of two resonators, or of two ear trumpets, placed at the ends of a long horizontal rod which rotates around a vertical axis. I may thus obtain an increase in the aural parallax. By rotating the horizontal rod around its center I may be able to bring the two sonorous sensations either to disappear, or to become of equal intensity, and by these indications to arrive at the direction of a sound. The last mentioned idea may develop into something useful to the mariner who has to ascertain the *direction* of a fog signal.

II. Botany and Zoology.

1. *Botanical Contributions*, separately issued from the current (eleventh) volume of the *Proceedings of the American Academy of Arts and Sciences.*—These are, first, a series of miscellaneous contributions, characters of new species, and several new genera, mainly Californian, by Asa Gray. The paper begins with the discrimination of two plants which have long been confounded, namely, *Sedum pusillum* of Michaux and *Diamorpha pusilla* of Nuttall. They grow together, but are distinct enough in appearance as well as in structure when seen in the living state, as they were by the author of this paper a year ago. There is a revision of the genus *Collinsia*, of the North American species of *Mimulus*, twenty-nine in number, and of *Monardella*, eleven in number.

The other papers are by Sereno Watson. 1. *On the Flora of Guadalupe Island*, Lower California, founded upon a unique collection of dried plants made by Dr. Edward Palmer. 2. List of the collection with Dr. Palmer's notes upon them. There is a notable amount of new species, and two new genera. These are characterized, as are the new *Gamopetalæ*, by Prof. Gray in his paper above mentioned, as to the remainder by Mr. Watson in his third article, entitled, *Descriptions of New Species of Plants chiefly Californian, with Revisions of certain Genera.* The other new species described and the revisions are mainly such as came to light in Mr. Watson's work on the *Polypetalæ* of the Botany of California, soon to be published. Among them are many plants of much interest, such, for instance as second species of the genera *Crossosma*, *Lyrocarpa* and *Adolphia*. *Cercidium* is shown to belong to *Parkinsonia.* A revision is made of the North American species of *Trifolium*, also of *Lathyrus* and *Peucedanum*, both very critical works; and the cucurbitaceous genus *Megarhiza* of Torrey is re-established upon five species. Mr. Watson's descriptions, as usual, are in the English language,—an advantage in home but not for foreign use. A. G.

2. *Botanical Necrology* of 1875. On the home list only one name recurs to memory, that of

INCREASE ALLEN LAPHAM, LL.D. He died September 14, in the 64th year of his age. A beautiful tribute to his memory, read before the Old Settler's Club, of Milwaukie, by S. S. Sherman, Esq., has just been printed, and is noticed on a following page. An excellent portrait is prefixed.

The following botanists have deceased in Europe:

FRIEDRICH GOTTLIEB BARTLING, one of the oldest professors at Göttingen, a veteran teacher, but not a voluminous author; aged 77.

ALEXANDRE BOREAU, of Angers, France, author of the Flora du Centre de la France.

JOHN EDWARD GRAY, March 7, at the age of 75. Principally known as a zöologist, some of his earliest work was in botany. A notice of his life and services appeared in this Journal, vol. x, p. 78.

JEAN CHARLES MARIE GRENIER, one of the authors of the classical Flora de France, died at Besançon, in the 69th year of his age.

DANIEL HANBURY, died at Clapham, March 24, in his 50th year. A notice appeared in this Journal, vol. ix, p. 75. We learn that his scattered writings are to be collected.

RUDOLPH FREIDRICH HOHENHACHER, died at Kirchheim in Wurtemburg, late in the preceding year, November 14, 1874. He was in early life a missionary at Astrakan, and was afterward in the Caucasian provinces. He was one of the founders of the Unio Itinerario, and he survived his associates, Steudel and Hochstetter.

LIEUT. GENERAL JACOBI, the monographer of *Agave*, died at Berlin, early in the year.

ERNST FERDIND NOLTE, of Kiel, a veteran botanist, who had retired from his professorship a year or two ago, died February 13, at the age of 84.

GUSTAVE THURET, died suddenly at Antibes, France, May 17, at the age of 58. A brief notice of this sad loss was given in vol. x, p. 67. Among other tributes to his memory is one by Rostafinski in the Botanische Zeitung, for July 30. Also one by Professor Farlow in Trimen's Journal of Botany, for January last.

ADOLPHE BRONGNIART. We learn that this veteran botanist and vegetable paleontologist died at Paris. A. G.

3. *Life Histories of Animals, including Man, or Outlines of Comparative Embryology;* by A. S. PACKARD, Jr. 8vo, 239 pp. with 268 cuts. New York, 1876. (Henry Holt & Co.)—This work consists of a series of papers published in the American Naturalist during the past year, with the addition of a few pages on mammals and nearly three on man. Although, according to the preface, the original papers have undergone "careful revision," we regret to notice many serious errors and inaccurate statements that have been allowed to remain, and are much less excusable in

the permanent book form than in mere magazine articles. Most of these errors must evidently be attributed rather to carelessness in the mode of statement than to lack of knowledge on the part of the author. Thus on page 187, after properly describing the zoëa of crustacea as having only "antennæ, jaws and foot-jaws" for locomotive appendages, he states that he has examined *Gelasimi* carrying eggs which "contained zoëæ, with the two claws alike, and it is probable that the strange inequality in size of the claws in these animals does not show itself until after one or more moults." What such an incongruous statement means can scarcely be imagined, for the "claws" and other legs are not even formed until a much later period, and it is well known that no such inequality exists in the adult females, nor even in the young males until after they become genuine little fiddler-crabs. On page 216 he says: "the tadpole is much less developed than the larval fish or any other vertebrate; the intestine is not yet formed," but in the next sentence he adds: "It is also a vegetarian, eating decaying leaves; the mouth is small and round, the alimentary canal is remarkably long, the intestine coiled up in a spiral, the mouth is small, destitute of a tongue." On page 157 he says of the Gephyrea: "In none of these worms are there bristles or indications of segments," forgetting the well-marked segments and conspicuous setæ of *Echiurus*, and the less numerous ones of *Thalassema* and other genera. On page 120 he says: "There is in the Annelids a dorsal and ventral blood-vessel, the circulatory apparatus being closed and more highly developed than in the Crustacea and Insects, *Limulus* excepted," but this is by no means true of all Annelids, for in many genera (*Polycirrus*, etc.,) there are no blood-vessels whatever, and many others have a very imperfect system of vessels. On page 117, fig. 126 is said to represent a later stage of *Loligo* than fig. 125, but the reverse is true. On page 78 it is said that "in the star-fishes and Holothurians, the alimentary canal opens into five voluminous cæcal appendages," and that these "are in connection with the complicated water tubes" (ambulacral tubes and suckers). The latter statement is entirely erroneous, or at least misleading, and the former is not true of most Holothurians, nor even of all star-fishes. The statement on page 79, that "in those star-fishes in which the alimentary canal is a blind sac, the eggs are emptied into the body cavity" is also incorrect, at least for most species. The statement (p. 70) that "in the Hydroids also the ovaries hang outside the body cavity" is inaccurate, as are also the statements (p. 63) that only *one case* of multiplication by fission has been observed among Hydroids, and on page 58, that the "ovaries" of *Hydra* "differ entirely in their mode of formation from the ovaries (gonophores) of the marine Hydroids, which are genuine buds." The account of *Physalia* (p. 65); that of the growth of septa in Polyps (p. 71); of the development of barnacles (p. 169), and many other paragraphs need revision. The peculiar mode of development of *Tubulariæ*, in which the embryo becomes an

"actinula" without going through a "planula" stage, is not alluded to, the planula stage being given as a stage of all Hydroids (p. 66). On page 336 "*Didelphia*" is used where *Monodelphia* is intended. The five diagrammatic figures on p. 225, given to illustrate birds (fowl), would serve better for mammalian embryology. The lists of works given at the end of each chapter are very incomplete and unsatisfactory, many of the latest and most important books being omitted from most of them. Thus under Hydroids the works of Hincks and Allman, both of which contain much of importance on embryology, are omitted. The magnificent treatise on Tubularians by the latter is certainly one of the most important hitherto published, both for the embryology and structure of Hydroids. The recent extensive work on the Nemerteans by McIntosh is not mentioned, though it contains the embryology of the group. Cobbold's works are only once alluded to, and are not mentioned under those groups of Helminths upon which he has done the most. The very valuable works of G. O. Sars on various Crustacea, including the discovery of the very remarkable phenomena in the embryology of *Cladocera*, are not referred to. Most of these are works written in English, and should be well known to the author of a work on embryology. Certainly references to such works would have been more useful for most of his readers than those that he gives to special papers in the German and Russian periodicals, which are generally inaccessible, however valuable they may be. In spite of these defects the book will doubtless prove to be a very useful one, there being no other work in English covering the same ground. V.

4. *On some Remarkable Forms of Animal Life from the great deeps off the Norwegian Coast. II. Researches on the Structure and Affinity of the genus Brisinga, based on the study of a new species, Brisinga coronata;* by GEORGE OSSIAN SARS, (University-Programme for the last half-year, 1875. Christiania).—In this valuable memoir, which is illustrated by seven excellent plates, Professor Sars has given a detailed description of the anatomy, physiology, and development of the genus *Brisinga*, perhaps the most remarkable form of star-fish hitherto discovered. The author also discusses, at considerable length, its relations to other star-fishes, recent and fossil, as well as to Echinoderms in general, and the relation of Echinoderms to the Annelids. He regards *Brisinga* as the most generalized form of star-fish, and consequently of Echinoderms, and supposes it to be one of the little-modified survivors of a primitive type from which the other forms of Echinoderms have descended. It has affinities to the most ancient fossil starfishes of the Palæozoic rocks (*Protaster*, etc.)

The existence of a genuine vascular system, distinct from the general perivisceral cavity and its extensions, is denied both in the case of this genus and of other star-fishes. The author also states that there is no anal orifice, although there is, as in other star-fishes, a dorsal gland, with a narrow duct opening on the dorsal surface, and he suggests that this duct has in other star-fishes been

mistaken for an intestine, and its outlet for an anus, the existence of which, in any star-fish, he doubts. Prof. Sars adopts the view, previously advanced by Duvernoy, Huxley, and Hæckel, that an Echinoderm is a cluster (or "comus") composed of several articulated zoöids ("persons") united by their anterior ends. V.

5. *A Course of Practical Instruction in Elementary Biology;* by T. H. HUXLEY, assisted by H. N. MARTIN. 268 pp. small 8vo, without illustrations. (Macmillan & Co.) 1875.—The publishers have seen fit to over-burden this otherwise excellent little manual with an exorbitant price ($2.50 in the U. S.), which will doubtless prevent its adoption in many cases where it should be used. It contains very full descriptions of the structure of a number of diverse forms of plants and animals, with plain and explicit directions how to dissect and study them. V.

6. *Crustacea of Mexico and Central America.* Mission Scientifique au Mexique et dans l'Amérique Centrale, publié par ordre du Ministre de l'Instruction Publique. *Etudes sur les Xiphosures et les Crustacés de la région Mexicane,* par M. ALPHONSE MILNE-EDWARDS. 4to, Paris: 1re et 2e livraisons, pp. 56, plates 1–14, 1873; 3e livr., pp. 57–120, plates 15–20, 1875.—These memoirs are published in the same elaborate and sumptuous style as the other works of this series, and are by far the most extensive and important contribution yet made to our knowledge of the crustaceans of the tropical region of America. The first forty pages and twelve plates are devoted to an elaborate study of the anatomy of Limulus, the substance of which had previously appeared in the Annales des Sciences Naturelles during the delay in the publication of the present work. The second memoir is devoted to a systematic account of the stalk-eyed Crustacea of the Mexican region, including Central America, Lower California, the Galapagos, and the West Indies. This memoir, beginning with the Maioidea, treats, thus far, of the *Pericerinæ*, *Pisinæ*, and *Mithracinæ*, including twenty-three genera, or sub-genera, and seventy-four species. A large proportion of the species are represented on the plates by beautiful figures of the entire animals, and numerous details. Twelve of the species and five of the genera are described as new. The author states, in the introduction, that a large part of the collections obtained by the Mexican Commission were destroyed, during the bombardment of Paris, by the explosion of a Prussian shell which had passed through the cases containing the conchological collections. On this account the work is based to a considerable extent on collections received from the United States, particularly from the Smithsonian Institution and the Museum of Comparative Zoology at Cambridge. S. I. S.

7. *Cumacea from great depths in the Arctic Ocean;* by G. O. SARS. 12 pp. 4to, with 4 plates. (From the Svenska Vetenskaps-Akademiens Handlingar, Bandet xi; Stockholm, 1873.)—This is the third of Professor Sars's richly illustrated memoirs on the Cumacea, and treats of five species from the Arctic Ocean. *Diastylis polaris* and *D. stygia* are from the remarkable depths of 950 and 2600 fathoms respectively. S. I. S.

8. *Moa or Dinornis of New Zealand.*—Remains of skeletons of fifteen Moas have been discovered along the beach, north of Whangarei Heads, sixty miles north of Auckland. Several human skulls and a complete human skeleton in sitting posture (the usual burying posture among the natives) were found with the Moa bones. Previously, no Moa bones had been found north of Auckland.—*Nature*, Feb. 3.

9. *Carnivorous Reptiles having some features of Carnivorous Mammals from the Triassic (?) of South Africa.*—Professor Owen has described, in a paper read before the Geological Society of London on February 2d, a carnivorous reptile, named by him *Cynodracon major*, which has the compressed sabre-shaped canines of the Lion of the genus *Machærodus*, and resembles Carnivores both in the canines and incisors. In the lower jaw the bases of eight incisors and of two canines (very inferior in size to the canines of the upper jaw) are visible, and the canines are separated by a diastema from the incisors. In this character, as in the number of incisors, the fossil resembles a *Didelphys*. "The left humerus is 10½ inches long, but is abraded at both extremities; it presents characters, in the ridges for muscular attachment, in the provision for the rotation of the forearm, and in the presence of a strong bony bridge for the protection of the main artery and nerve of the forearm, which resemble those occurring in carnivorous mammals, and especially in the Felidæ, although these peculiarities are associated with others having no mammalian resemblances." "Prof. Owen discusses these characters in detail, and indicates that there is, in the probably Triassic lacustrine deposits of South Africa, a whole group of genera (including *Galesaurus*, *Cynochampsa*, *Lycosaurus*, *Tigrisuchus*, *Cynosuchus*, *Nythosaurus*, *Scaloposaurus*, *Procolophon*, *Gorgonops and Cynodracon*), many represented by more than one species, and all carnivorous, which have more or less decided mammalian analogies; and to them he gives the general name of *Theriodonts*.

The common characters of the Theriodonts are as follows: dentition of the carnivorous type; incisors defined by position, and divided from the molars by a large laniariform canine on each side of both jaws, the lower canine crossing in front of the upper; no ecto-pterygoids; humerus with an entepicondylar foramen; digital formula of the fore foot 2, 3, 3, 3, 3 phalanges.—*Proc. Geol. Soc. of Feb.* 2, *in Ann. Mag. Nat. Hist.*, for March, 1876.

10. *The Crustacean, Artemia salina, changed in some of its characters by changing the saltness of the water in which it lives.*—W. J. Schmankewitsch announces, that by increasing the saltness of the water in which the *Artemia salina* lives, a modification goes on from generation to generation, until the caudal lobes finally disappear, and the form is that in the *Artemia Mühlhausenii*; and by reversing the process, the caudal lobes grow out again and become those of *A. salina*. In 1871 the salt marshes about Odessa contained great numbers of *A. salina*; the waters then marked only 8° Baumé. Afterward, on the repair of a dyke,

the saltness increased to 14° Baumé in the summer of 1872, and 25°, in August, 1874. The changes in the species were then first noted. The author afterward corroborated the fact by experiments on Artemiæ reared in captivity in water of which the saltness was gradually increased. A change also takes place, correspondingly, in the form of the branchiæ, and in the number of apodal segments. —*Ann. Mag. N. H.*, March, 1876, from *Zeitschr. wiss. Zool.*, xxv, Suppl. i, 1875, p. 103, pl. 6.

III. Astronomy.

1. *Astronomical and Meteorological Observations made during the year* 1873, *at the U. S. N. Observatory, with Appendix;* Rear-Admiral B. F. Sands, Superintendent. Gov. Printing Office.

This volume contains the record of a year's work at the Observatory, and is evidently the record of first rate work. The Appendix by Professor Newcomb has been already noticed in our February Number.

If the use for several years of one value of the latitude of the Observatory for reducing observations, made by the mural circle, and of another value for observations made by the transit circle, were the deliberate choice of the Superintendent, astronomers would probably think his decision unwise. The same also must be said of using a different latitude each year for the final tables of north polar distances, derived from the observations of the transit circle. It would also look better, to say the least, if the results were given in the same denomination, instead of north-polar distance for one instrument, and declination for the other.

These things look not so much like the deliberate choice of the Superintendent, as the kind of little irregularities that must be expected from a system that makes little account of scientific fitness in appointing the Superintendent of a scientific institution. We hope for better things from the present Superintendent. H. A. N.

2. *Auxiliary Tables for determining the angle of position of the sun's axis and the latitude and longitude of the Earth referred to the Sun's equator:* by Warren De La Rue; 20 pp. 4to, London, 1875. Printed for private circulation.

IV. Miscellaneous Scientific Intelligence.

1. *Third Report of the Settle Caves (Victoria Cave) Committee of Exploration;* by R. H. Tiddeman. (Rep. Brit. Assoc. 1875.) —Mr. Tiddeman was Secretary of the Committee in charge of the exploration, the other members being Sir John Lubbock, Prof. Hughes, Prof. Dawkins, and Mr. L. C. Miall. The Victoria Cave afforded the preceding year, among remains of *Ursus spelæus*, *U. ferox*, *Hyæna*, *Rhinoceros hemitœchus*, *Bison*, *Cervus elephas*, and molars of *Elephas antiquus*, a bone pronounced by Prof. Busk to be a human fibula. Thick bowlder deposits covered the entrance to the cavern, and hence the remains were pronounced to

be pre-Glacial. This *third* Report states that great progress had finally been made in uncovering the glacial deposits at the entrance, in which were bowlders of all sizes up to several tons in weight. The question whether "these glacial deposits, which rest upon the older bone-beds containing the remains of extinct mammals and man, are in the position which they occupied at the close of the Glacial conditions, or have subsequently fallen into their present site," is answered by stating that the new facts "go to prove the first alternative." In one chamber (numbered D) the upper bone-bed afforded remains of the *Badger*, *Horse*, *Pig*, *Reindeer*, *Goat* or *Sheep*, and was peculiar in the abundance of *Reindeer* remains and the absence of the Elephant, Rhinoceros, Hippopotamus, Hyæna, as if it were of the Reindeer epoch, or later; the lower afforded bones of *Hyæna*, *Brown Bear*(?), *Elephas antiquus*, *Rhinoceros hemitœchus*, *Hippopotamus*, *Bos primigenius;* while, in both, there occur remains of *Man*, *Fox*, *Grisly Bear* and *Red Deer*. A piece of a human rib was found during the year in the lower bed, near where the fibula was taken out.

2. *Air and its Relations to Life;* by WALTER NOEL HARTLEY, F.C.S., Kings College, London. 263 pp. 12mo. New York, 1875. (D. Appleton & Co.)—This very readable little volume contains the substance of a course of six summer lectures delivered in 1874 at the Royal Institute of Great Britain. The author exhibits the rare faculty of presenting the results of exact science in a form perfectly intelligible and attractive to intelligent people not familiar with the technical language of science. The researches of the most trustworthy investigators are cited with good judgment from the days of Black and Lavoisier to those of Retenkofer, Angus Smith and Pasteur. Indeed it is not easy to say where else in English we can find so full a statement of the researches of Pasteur as in chapter four of Mr. Hartley's essay.

3. *Geological and Geographical Survey of the Territories*, Prof. F. V. HAYDEN *in charge*.—Bulletin No. 1, Vol. II, of this Survey has appeared It contains seven articles. Three, by, severally, Messrs. Holmes, Jackson and Bessels, treat of the Ancient Ruins of Southwestern Colorado, Utah and Arizona, and are illustrated with twenty-nine octavo plates, of cliff dwellings and other ruins, pottery, utensils, crania, etc. Of the remaining four, three are short articles on the Ute Indians, by E. A. Barber; and a fourth consists of descriptions of thirty-one new species of fossil Coleoptera from the Tertiary formations of the West, by S. H. Scudder. The volume is full of interesting facts in American Archæology, and the maps and plates illustrate well the subjects discussed.

3. *Compressed Peat.*—Peat pressed into blocks and made so compact that a cubic foot weighs 85 to 100 pounds, is manufactured by Mr. A. E. Barthel, of Detroit, Michigan, and sells for one and a half dollars per ton.

4. *Report of the Superintendent of the U. S. Coast Survey*, showing the progress of the Survey during 1872. This report contains 18 appendixes, among which we note the report of Assis-

tant Cutts and Prof. Young of Astronomical and Meteorological observations made at Sherman, Wyoming Ter., pp. 75–172; and a preliminary report on transatlantic longitude, by Prof. Hilgard, pp. 227–234.

Notices of the following new publications have been excluded from this number by want of space.

Geological Survey of Ohio. Paleontology, vol. ii. 436 pp. 8vo, with numerous plates. A volume of great value.

Mineral Resources West of the Rocky Mountains. 7th Annual Report, by R. W. Raymond. 540 pp. 8vo. Washington, 1875.

Second Geological Survey of Pennsylvania. Report of Progress in the Clearfield and Jefferson District of the Bituminous Coal-fields of Western Pennsylvania, by Franklin Platt. 296 pp. 8vo, with 139 wood-cuts and 10 maps and sections. Harrisburg, Pa., 1874.

Bulletin of the U. S. National Museum. No. 1 by E. D. Cope, and No. 2 by J. H. Kidder. Smithsonian Institution, 1875.

Report on the Geology of a portion of Colorado, by John J. Stevenson. 372 pp. 4to, 1876. Reprinted from Lieut. Wheeler's Survey Report, vol. iii.

Report of the Chief Signal Officer, War Department, 1875. 476 pp. 8vo, with numerous maps.

OBITUARY.

I. A. LAPHAM, LL.D.: a Biographical Sketch, by S. S. Sherman. 80 pp. 8vo, 1876.—This memoir is a very just tribute to the memory of Dr. Lapham, who died at Milwaukee, Wisconsin, in September, as already stated in this Journal. Dr. Lapham was a man of very varied knowledge and scientific labors. Early in life, while engaged in engineering duties, he began a collection of plants, which at his death numbered 8,000 species, and he also published papers on the geology of portions of Ohio. Moving to Milwaukee in 1836, he commenced observations on the topography, soil, mineral and other industrial resources, of Wisconsin, and on the commerce and navigation of the lakes, and kept tables of the daily temperature, rain-fall, and other meteorological phenomena; and in 1844 he published for Wisconsin a volume of 250 pages, on these topics. He afterward contributed Agricultural, Botanical and Geological papers to the Transactions of the Wisconsin State Agricultural Society, among them a valuable treatise of nearly 100 pages on "The Grains of Wisconsin and adjacent States," a paper which he afterward extended to a manuscript volume of 574 pages on the Gramineæ of the United States, but which remains unpublished. The fluctuations in the level of Lake Michigan early engaged his attention, and in 1849 he announced his discovery of "a slight lunar tide in Lake Michigan." The study of Indian mounds of Wisconsin occupied much of his time, and as early as 1836 he called attention to a turtle-shaped mound at Waukesha. He was the first to notice that many of these aboriginal earthworks are "gigantic basso-relievos of men, beasts, birds and reptiles." His well known and highly valued "Antiquities of Wisconsin," printed by the Smithsonian Institution, is a large quarto volume, containing 55 plates and numerous wood-engravings, all from his own

drawings. One of his last labors was the preparation of a series of models of the Indian mounds for the Centennial Exhibition of 1876.

Another subject which occupied him was Meteorites; and peculiar crystalline markings in a Wisconsin meteorite, first noticed by him, were designated by Dr. J. Lawrence Smith, in a paper on the meteorite in this Journal (II, xlvii, 271), *Laphamite markings.*

The establishment of the Signal Service Bureau at Washington in 1869 was due largely to personal effort and influence on the part of Mr. Lapham.

Dr. Lapham was placed, in 1873, at the head of the Geological Survey of the State of Wisconsin, a position for which he was well fitted: and the Survey went forward with energy and important results through that year and 1874. To the misfortune of science and the State, he was deposed at the close of 1874, and, through political management, a man ignorant of geology was substituted. It was a serious disappointment to Dr. Lapham, and not less so to all friends of science in the land. "His abrupt dismissal was all the more cruel because this was the only opportunity he ever had of perfecting and giving to the public in a permanent form the results of a life-work in the geology, natural history and industrial resources of the State."

Dr. Lapham was active also in all educational movements; a founder of the Milwaukee Female College, a liberal contributor to the Cabinet of the Wisconsin State University, and one of the founders of the Wisconsin Historical Society, and of the Wisconsin Academy of Sciences.

Rev. Augustus Wing, of Rochester, Vermont, died in Whiting, in that State, on the 19th of January, aged sixty-seven years. Mr. Wing was a graduate of Amherst College, of the class of 1835. Although not a geologist by profession, a large part of his time for many years had been spent in the study of the rocks of Vermont, and especially of the crystalline limestone, quartzyte and slates of the central portion of the State. By the discovery of Lower Silurian fossils in the crystalline limestone at several different localities he threw much light on the geology of metamorphic New England. In August of the past year the writer had the pleasure of accompanying Mr. Wing on a visit to some of his localities that were of special interest for their fossils or for their illustration of the stratification of the rocks, and this was his last scientific excursion, excepting one of a few days during the following fortnight. His almost child-like delight over the places of his remarkable discoveries as he pointed them out; his earnestness in making known his conclusions and in supporting them against all expressed doubts; his eager, rapid gait as we walked over the rocks and hills, made him an especially agreeable companion, and suggested no thought of the end that was so soon to come. Before parting, he promised to send for this Journal an account of his discoveries, as in fact he had done before. But he disliked writing, and it was not sent. We hope that his notes may yet afford material for such a paper.

J. D. D.

APPENDIX.

ART. XLI.—*Principal Characters of the* BRONTOTHERIDÆ; by O. C. MARSH. With four plates.

THE remains of a well-marked group of gigantic mammals are abundant in the lowest deposits of the Miocene, on the eastern slope of the Rocky Mountains. These animals, which have been named by the writer, *Brontotheridæ*, equaled the Eocene *Dinocerata* in size, and resembled them in some important features. They do not, however, belong to the same order, but constitute a distinct family of Perissodactyles. Four genera of this family are now known, as shown below, but *Brontotherium* is the only one represented by sufficient remains to clearly indicate its structure and affinities; and hence this genus will be first described, and mainly used to illustrate the group.

Brontotherium Marsh, 1873.*

The skull in *Brontotherium* is long and depressed, and resembles that of *Rhinoceros*. The occipital region is extended vertically, and deeply concave posteriorly. The vertex is concave longitudinally, and convex transversely. The general form of the skull is shown in the cut given below, figure 1.

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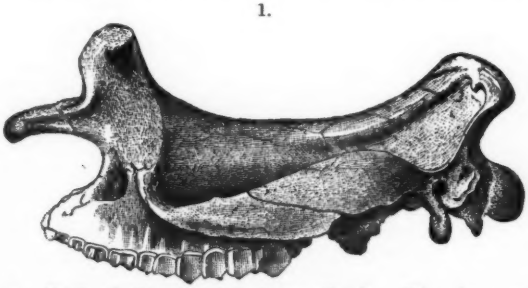

Figure 1. Skull of *Brontotherium ingens* Marsh. Side view; one-twelfth natural size.

There is a pair of large horn-cores on the anterior part of the skull, in front of the orbits. They stand on the maxillaries,

* This Journal, vol. v, p. 486. Also vol. vii, p. 81, Jan., 1874, and vol. iv, p. 245, March, 1875.

like the middle pair in *Dinoceras*, the nasals forming only the inner margin of the base. These protuberances are placed transversely, as in modern Artiodactyles, and extend upward and outward. They vary much with age, and probably differed with the sex. There are large air cavities in the base of these horn-cores. The nasal bones are greatly developed, and firmly co-ossified. Their anterior extremities are produced, and overhang the large narial orifice. The premaxillaries are diminutive, and do not usually extend forward so far as the end of the nasals. The infra-orbital foramen is very large. The lachrymal forms the anterior border of the orbit. The latter is small, and continuous with the elongated temporal fossa. There is no postorbital process on the frontal. The zygomatic arches are massive, and much expanded. The malar extends forward beyond the lower margin of the orbit. The zygomatic process of the squamosal is elevated, and more or less incurved above. There is a large postglenoid process, which forms the anterior border of the external auditory meatus. The latter is bounded behind and below by the post-tympanic process of the squamosal. There is a large par-occipital process. The occipital condyles are large, and well separated. Their position indicates that the head was declined when in its natural position. There is a large condylar foramen, and a distinct alisphenoid canal. The palate is deeply excavated, especially in front. The posterior nares extend forward between the last upper molars.

The brain cavity in *Brontotherium* is small, and its form is shown in Plate XI, the figures of which are drawn from a nat-

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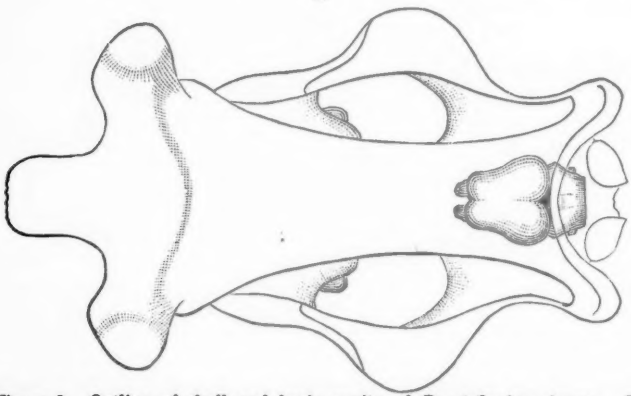


Figure 2. Outline of skull and brain cavity of *Brontotherium ingens*. Top view; one-tenth natural size.

ural cast of the brain-case of *B. ingens* Marsh. The size of the entire brain compared with that of the cranium is shown in the accompanying cut, figure 2.

The cerebral hemispheres did not extend at all over the cerebellum, and little if any over the olfactory lobes. The latter were of moderate size, and separated by a wide osseous septum. The hemispheres were comparatively large, and much convoluted. The Sylvian fissure is well-marked in the cast, and some of the other principal divisions are indicated. The cerebellum was small. There was a rudimentary tentorial ridge. The pituitary fossa is distinctly marked. The foramina for the optic nerves are quite small.

The mandible in *Brontotherium* has a wide condyle, and a slender coronoid process. The angle is rounded, and slightly produced downward. The symphysis is depressed, elongated, very shallow in front, and completely ossified. (Plate XII.)

The dental formula of *Brontotherium* is as follows:—

$$\text{Incisors},\frac{2}{2};\ \text{canines},\frac{1}{1};\ \text{premolars},\frac{4}{3};\ \text{molars},\frac{3}{3}\times 2=38.$$

The upper incisors are quite small. (Plate X.) The canine is short and stout, and placed close to the first premolar. The upper premolars have all essentially the same structure, viz: two external connate cusps, with their outer faces nearly plane, and two inner cones closely united. The anterior cone is connected with the opposite outer cusp by a transverse ridge, which has behind it an elongated depression, more or less divided by projections from the outer posterior cusp. In the upper true molars, the external cusps have their outer surfaces deeply concave, while the inner cones are low and separate. The lower incisors were small, and evidently of little use. The two next the symphysis were separated from each other. The lower incisors are not unfrequently wanting, and in old animals the alveoli may, perhaps, disappear. Careful examination, however, will usually show indications of them. The lower canine is of moderate size, and separated from the premolars by a short diastema. The lower molars are of the *Palæotherium* type, and agree essentially with those of *Menodus*.

The neck in *Brontotherium* was stout, and of moderate length. The cervical and most of the dorsal vertebræ are distinctly opisthocœlous. The atlas is large, and much expanded transversely. The axis is massive, and has its anterior articular faces much broader than in the *Dinocerata*. The odontoid process was stout and conical. The posterior articular face is concave, and oblique. The transverse processes apparently had no foramen for the vertebral artery. The epiphyses of the vertebræ are loosely united in most specimens, as in the Proboscidians. The lumbars are slender, and smaller than the dorsals. There are four vertebræ in the sacrum. The caudal vertebræ indicate a long and slender tail.

The limbs of the *Brontotheridæ* were intermediate in proportion between those of the Elephant and the Rhinoceros. The scapula is large, with a prominent spine and small coracoid process. The humerus is stout, and its great tuberosity extends above the head. The radial crest is prominent, and the entire distal end is occupied by the articulation. The olecranon cavity is shallow, and the condylar ridge similar to that of the Elephant, but not continued so far up the shaft. The radius and ulna are separate. The ulna has its olecranon portion much compressed. Its distal end is much smaller than in *Rhinoceros*, and has no articular face for the lunar. The radius is stout, and its distal end expanded. The carpal bones form interlocking series. They are shorter than in *Rhinoceros*, and support four well developed toes of nearly equal size. (Plate XIII, figure 2.) The metacarpal bones are shorter than those of *Rhinoceros*, the first phalanges longer, and the second series shorter. All the toes had "navicular" sesamoid bones, similar to that on the coronary bone of the horse. The ungual phalanges are short and tubercular, as in the *Dinocerata* and *Proboscidea*.

The pelvis is much expanded transversely. The femur has a small third trochanter, and its head a deep pit for the round ligament. At the distal end, the anterior articular surface is narrow, and the two edges are of nearly equal prominence, as in the Tapir. The patella is elongate, and has a strong vertical keel on its articular face. The tibia is stout, and has a distinct spine. The fibula is separate and entire, but quite slender. The calcaneum is much elongated. The astragalus is shorter than in the Rhinoceros, and the superior groove more oblique. The cuboid face is larger than in *Rhinoceros*. The navicular has its distal facets subequal. There were three toes of nearly equal size in the pes, the first and fifth being entirely wanting. (Plate XII, figure 1.) None of the bones of the skeleton are hollow.

There appear to be four well marked genera in the *Brontotheridæ*, now known, which may be distinguished as follows:

1. *Menodus* Pomel.* (*Titanotherium* Leidy, 1852.)

$$\text{Dentition} = \text{Incisors}\frac{2}{2};\ \text{canines}\frac{1}{1};\ \text{premolars}\frac{4}{4?};\ \text{molars}\frac{3}{3}.$$

Diastema behind upper canines. Basal ridge on inner side of upper premolars not continuous. Nasals short. A postorbital process. Third trochanter rudimentary or wanting. Type *M. Proutii.*

2. *Megacerops* Leidy. (*Megaceratops* Cope), (*Symborodon* Cope in part.)

$$\text{Dentition} = \text{Incisors}\frac{2}{0};\ \text{canines}\frac{1}{1};\ \text{premolars}\frac{4}{3};\ \text{molars}\frac{3}{3}.$$

* Bib. Univ. de Genève, x, p. 75, Jan., 1849.

Diastema behind upper canines. Inner basal ridge on upper premolars not continuous. Nasals more elongated. A postorbital process. Third trochanter rudimentary or wanting. Type *Megacerops Coloradensis* Leidy.

3. *Brontotherium* Marsh, (*Symborodon* Cope, in part.) (*Miobasileus* Cope.)

Dentition $=$Incisors$\frac{2}{2}$; canines$\frac{1}{1}$; premolars$\frac{4}{3}$; molars$\frac{3}{3}$.

No superior diastema. Strong continuous basal ridge on inner side of upper premolars. No postorbital process. Third trochanter distinct. Type *B. gigas* Marsh.

4. *Diconodon* Marsh (*Anisacodon*).

Dentition $=$Incisors$\frac{0}{1}$; canines$\frac{1}{1}$; premolars$\frac{4}{3}$; molars$\frac{3}{3}$.

No superior diastema. Strong inner basal ridge on upper premolars. Last upper molar with two inner cones. No postorital process. Type *D. montanus* Marsh.

In the dentition and skeleton, the *Brontotheridæ* more nearly resemble the Eocene *Diplacodon*, than any other American genus, and they may yet prove to be nearly related. The animals of that genus were of much smaller size, and entirely without horns. The relations of the *Brontotheridæ* to the genus *Chalicotherium* Kaup, cannot at present be determined.

In comparing the *Brontotheridæ* with the equally gigantic *Dinocerata* of the Eocene, several striking points of resemblance will be at once noticed; especially the presence of horn-cores in transverse pairs: the general structure of the limbs; and the short and thick toes. The differences, however, between these two groups are still more marked. In the *Brontotheridæ* there is but a single pair of horn-cores, and no crest around the vertex. The structure and number of the teeth are quite different, while the small canines and huge molars contrast strongly with the elongated canine tusks and diminutive molars of the *Dinocerata*. The latter, moreover, have two very large dependent processes on each ramus of the mandible; the cervical vertebræ flat; the femur without a third trochanter; and at least an additional toe in each foot.

Among the features which this group shares with the *Proboscidea* may be mentioned: the superior extension of the condylar ridge of the humerus; the short thick toes; and the late union of the epiphyses with the centra of the vertebræ. The last character appears to belong especially to mammals of very large size, and probably indicates late maturity, and great longevity.

The *Brontotheridæ* nearly equaled the Elephant in size, but the limbs were shorter. The nose was probably flexible, as in the Tapir, but there was evidently no true proboscis.

All the known remains of the *Brontotheridæ* are from east of the Rocky Mountains, in the Miocene beds of Dakota, Nebraska, Wyoming, and Colorado.

Yale College, New Haven, March 16, 1876.

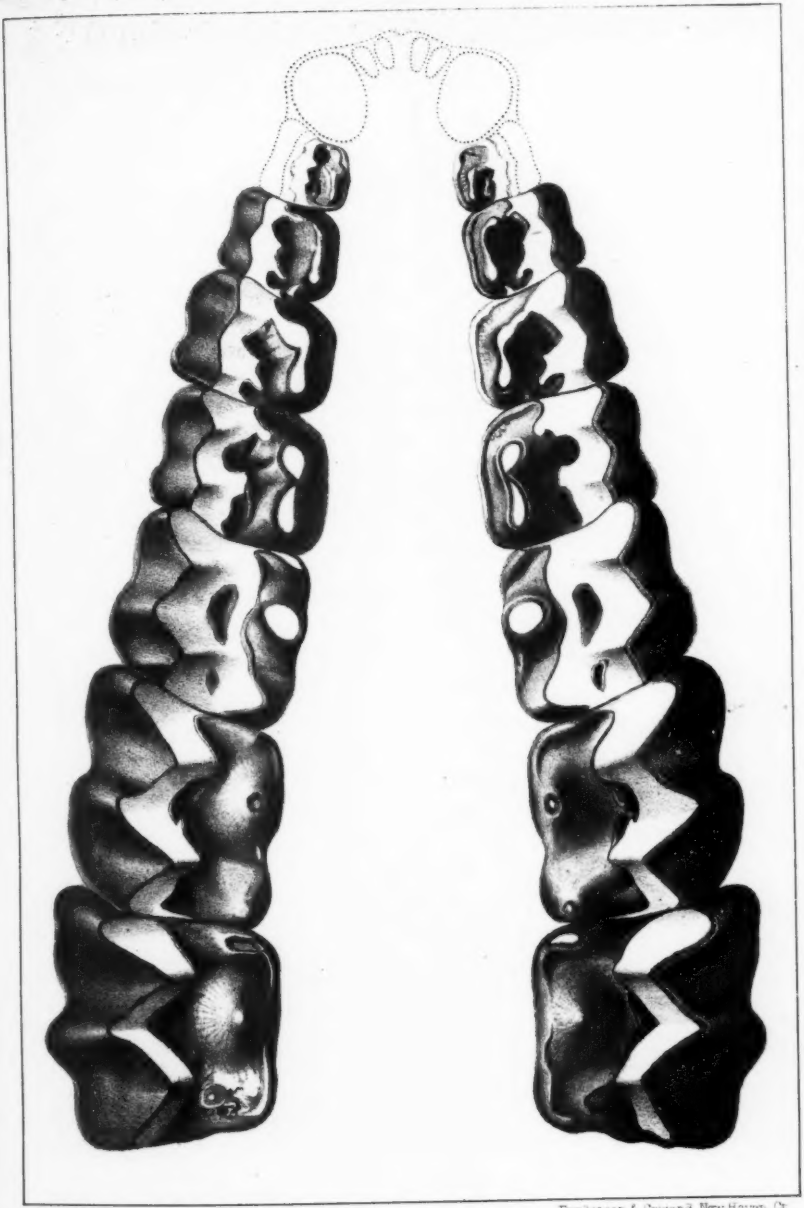
EXPLANATION OF PLATES.

Plate X—*Brontotherium ingens* Marsh. Superior premolar and molar teeth; bottom view. One-third natural size.

Plate XI—*Brontotherium ingens.* Cast of brain cavity. Figure 1, top view; figure 2, side view. One-half natural size.

Plate XII—*Brontotherium gigas* Marsh. Lower jaw. Figure 1, top view; figure 2, front view; figure 3, side view. One-sixth natural size.

Plate XIII—*Brontotherium.* Figure 1, hind foot; figure 2, fore foot. One-sixth natural size.



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BRONTOTHERIUM INGENS. Marsh. 1/8

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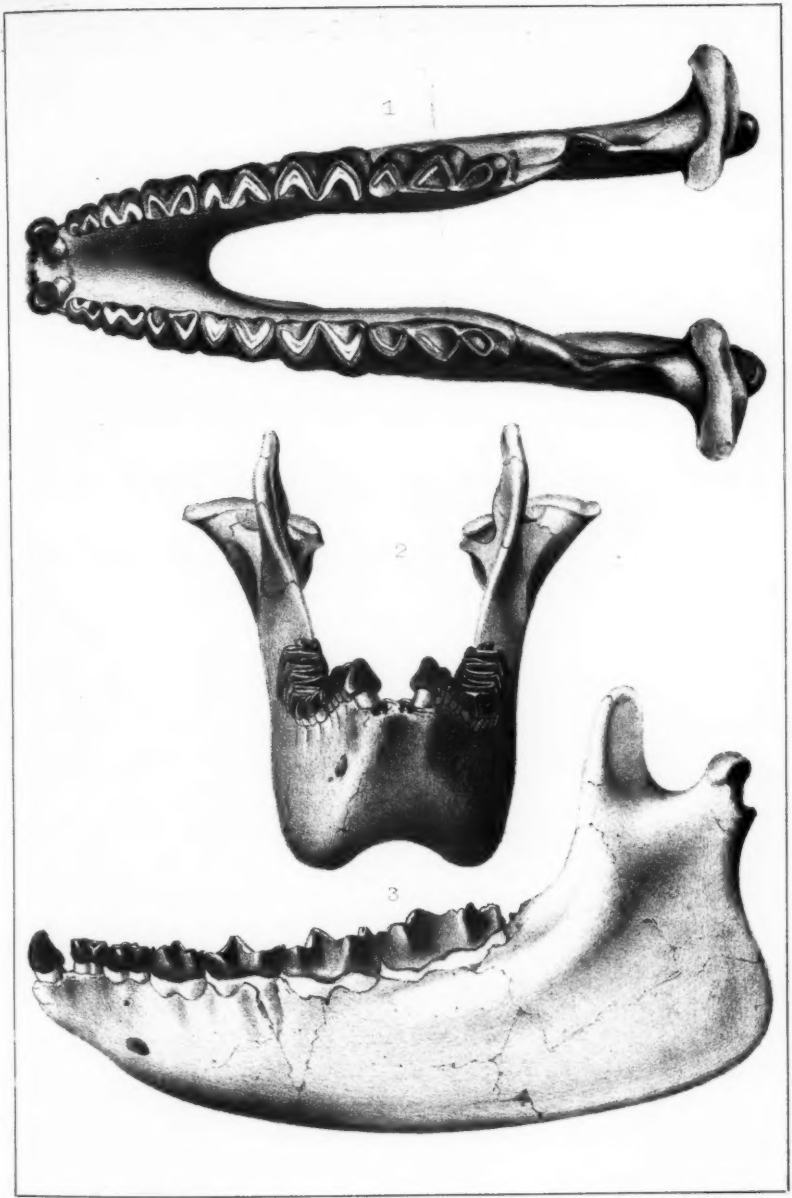

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BRONTOTHERIUM GIGAS. Marsh 1/6.

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BRONTOTHERIUM.